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PROCEEDINGS

OF

THE ROYAL SOCIETY.

November 15, 1888.

Professor G. G. STOKES, D.C.L., President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Mr. John Ball, Sir James Cockle, Dr. Huggins, Dr. Rae, and Mr. Symons were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Combustion in dried Oxygen." By H. BRERETON BAKER, M.A., Dulwich College, late Scholar of Balliol College, Oxford. Communicated by Professor H. B. DIXON, F.R.S. Received July 4, 1888.

(Abstract.)

In 1884 some preliminary experiments, published in the 'Journal of the Chemical Society,' convinced me that moisture exerted an important influence on the combustion of carbon. Since that time experiments have been made, not only with that element but with several others, and the same influence seems to be exerted on the combustion of some, while no such influence could be detected in the case of other elements. It was discovered very early in the investigation that hydrogen, both free and combined, aided the union of carbon with dried oxygen, and therefore for the new experiments on this and other elements, special attention was devoted to their purification from hydrogen. It was found that two of these elements, amorphous phosphorus and boron, had, like carbon, a very great power of occluding hydrogen. To eliminate it some of the elements were heated in a current of pure chlorine, while others were heated in sealed tubes with the chlorides of the elements, special precautions being taken to

free the purified elements from all traces of the agents used in their purification. In this way the elements—carbon, sulphur, boron, and phosphorus, the latter in both red and yellow modifications—were found to have their combustion influenced by the dryness of the oxygen. Some chemical union was found to take place, the extent of which varied with the dryness of the substances. In no case, however, did it manifest itself by flame. Ordinary phosphorus was obtained so pure as not to glow in the oxygen dried by phosphorus pentoxide, though the pressure was increased and diminished in every possible way. If water was added rapid combustion at once set in.

The elements—selenium, tellurium, arsenic, and antimony—were purified with as much care as was expended on the elements mentioned above. Their combustion was, however, not found to be affected in any way by the dryness of the gas.

In the course of the investigation two facts were discovered about the combustion: (i) of amorphous phosphorus, and (ii) of charcoal in oxygen. Amorphous phosphorus is generally regarded as being incapable of true combustion. It is asserted that before amorphous phosphorus can be heated to its kindling point, it changes into ordinary phosphorus, which then burns. This has been proved not to be the case. Amorphous phosphorus was heated in a current of nitrogen, free from traces of oxygen, to 260°, 278°, and 300°, in three experiments, without undergoing any change to the ordinary modification. If moist oxygen was substituted for the nitrogen combustion took place at 260°. It seems, therefore, probable that amorphous phosphorus undergoes a true combustion in oxygen without previous change to the ordinary modification.

With regard to the combustion of carbon, it has always been a doubtful question which of the two oxides is first formed. Is carbon monoxide the first product, undergoing further oxidation to the dioxide, or is carbon dioxide the first and only substance formed? The problem seems incapable of direct solution. It is, however, open to indirect attack. When carbon is heated in a current of oxygen dried for a short time by phosphorus pentoxide, a slow combustion goes on, and, though the oxygen is in excess, both oxides are produced. The amount of monoxide, however, is twenty times the amount of the dioxide. Experiments also show that this occurs at temperatures at which dry carbon dioxide is not reduced by carbon. The carbon monoxide must, therefore, be produced by the direct union of its elements, its further oxidation being prevented by the dryness of the gases. Confirmatory experiments were performed in which carbon monoxide was found to be produced by the slow combustion of carbon in air at 440°, a temperature too low for the reduction of the dioxide by carbon. It is probable that the ordinary combustion of carbon goes on in two stages, that carbon monoxide is

first produced, and, if circumstances are favourable, this is further oxidised to carbon dioxide.

II. "On the Mechanical Conditions of a Swarm of Meteorites, and on Theories of Cosmogony." By G. H. Darwin, LL.D., F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge. Received July 12, 1888.

(Abstract.)

Mr. Lockyer writes in his interesting paper on Meteorites* as follows:—

"The brighter lines in spiral nebulæ, and in those in which a rotation has been set up, are in all probability due to streams of meteorites with irregular motions out of the main streams, in which the collisions would be almost *nil*. It has already been suggested by Professor G. Darwin ('Nature,' vol. 31, p. 25)—using the gaseous hypothesis—that in such nebulæ 'the great mass of the gas is non-luminous, the luminosity being an evidence of condensation along lines of low velocity according to a well-known hydrodynamical law. From this point of view the visible nebula may be regarded as a luminous diagram of its own stream-lines.'"

The whole of Mr. Lockyer's paper, and especially this passage in it, leads me to make a suggestion for the reconciliation of two apparently divergent theories of the origin of planetary systems.

The nebular hypothesis depends essentially on the idea that the primitive nebula is a rotating mass of fluid, which at successive epochs becomes unstable from excess of rotation, and sheds a ring from the equatorial region.

The researches of Roche† (apparently but little known in this country) have imparted to this theory a precision which was wanting in Laplace's original exposition, and have rendered the explanation of the origin of the planets more perfect.

But notwithstanding the high probability that some theory of the kind is true, the acceptance of the nebular hypothesis presents great difficulties.

Sir William Thomson long ago expressed to me his opinion that the most probable origin of the planets was through a gradual accretion of meteoric matter, and the researches of Mr. Lockyer afford actual evidence in favour of the abundancy of meteorites in space.

* 'Nature,' Nov. 17, 1887. The paper itself is in 'Roy. Soc. Proc.,' Nov. 15, 1887 (No. 259, p. 117).

† 'Montpellier, Acad. Sci. Mém.'

But the very essence of the nebular hypothesis is the conception of fluid pressure, since without it the idea of a figure of equilibrium becomes inapplicable. Now, at first sight, the meteoric condition of matter seems absolutely inconsistent with a fluid pressure exercised by one part of the system on another. We thus seem driven either to the absolute rejection of the nebular hypothesis, or to deny that the meteoric condition was the immediate antecedent of the sun and planets. M. Faye has taken the former course, and accepts as a necessary consequence the formulation of a succession of events quite different from that of the nebular hypothesis.* I cannot myself find that his theory is an improvement on that of Laplace, except in regard to the adoption of meteorites, for he has lost the conception of the figure of equilibrium of a rotating mass of fluid.

The object of this paper is to point out that by a certain interpretation of the meteoric theory we may obtain a reconciliation of these two orders of ideas, and may hold that the origin of stellar and planetary systems is meteoric, whilst retaining the conception of fluid pressure.

According to the kinetic theory of gases fluid pressure is the average result of the impacts of molecules. If we imagine the molecules magnified until of the size of meteorites, their impacts will still, on a coarser scale, give a quasi-fluid pressure. I suggest then that the fluid pressure essential to the nebular hypothesis is in fact the resultant of countless impacts of meteorites.

The problems of hydrodynamics could hardly be attacked with success, if we were forced to start from the beginning and to consider the cannonade of molecules. But when once satisfied that the kinetic theory will give us a gas, which, in a space containing some millions of molecules, obeys all the laws of an ideal non-molecular gas filling all space, we may put the molecules out of sight and treat the gas as a plenum.

In the same way the difficulty of tracing the impacts of meteorites in detail is insuperable, but if we can find that such impacts give rise to a quasi-fluid pressure on a large scale, we may be able to trace out many results by treating an ideal plenum. Laplace's hypothesis implies such a plenum, and it is here maintained that this plenum is merely the idealisation of the impacts of meteorites.

As a bare suggestion this view is worth but little, for its acceptance or rejection must turn entirely on numerical values, which can only be obtained by the consideration of some actual system. It is obvious that the solar system is the only one about which we have sufficient knowledge to afford a basis for discussion. The paper, of which this is an abstract, is accordingly devoted to a consideration of the

* 'Sur l'Origine du Monde,' Paris, Gauthier-Villars, 1884. 'Annuaire pour l'an 1885, Bureau des Longitudes,' p. 757.

mechanics of a swarm of meteorites, with special numerical application to the solar system.

When two meteoric stones meet with planetary velocity, the stress between them during impact must generally be such that the limits of true elasticity are exceeded, and it may be urged that a kinetic theory is inapplicable unless the colliding particles are highly elastic. It may, however, I think, be shown that the very greatness of the velocities will impart what virtually amounts to an elasticity of a high order of perfection.

It appears, *à priori*, probable that when two meteorites clash, a portion of the solid matter of each is volatilised, and Mr. Lockyer considers the spectroscopic evidence conclusive that it is so. There is no doubt enough energy liberated on impact to volatilise the whole of both bodies, but only a small portion of each stone will undergo this change. A numerical example is given in the paper to show the enormous amount of energy with which we are dealing. It must necessarily be obscure as to how a small mass of solid matter *can* take up a very large amount of energy in a small fraction of a second, but spectroscopic evidence seems to show that it does so; and if so, we have what is virtually a violent explosive introduced between the two stones.

In a direct collision each stone is probably shattered into fragments, like the splashes of lead when a bullet hits an iron target. But direct collision must be a comparatively rare event. In glancing collisions the velocity of neither body is wholly arrested, the concentration of energy is not so enormous (although probably still sufficient to effect volatilisation), and since the stones rub past one another, more time is allowed for the matter round the point of contact to take up the energy; thus the whole process of collision is much more intelligible. The nearest terrestrial analogy is when a cannon-ball rebounds from the sea. In glancing collisions fracture will probably not be very frequent.

From these arguments it is probable that, when two meteorites meet, they attain an effective elasticity of a high order of perfection; but there is of course some loss of energy at each collision.

[It must, however, be admitted that on collision the deflection of path is rarely a very large angle; but a succession of glancing collisions would be capable of reversing the path, and thus the kinetic theory of meteorites may be taken as not differing materially from that of gases.*]

Perhaps the most serious difficulty in the whole theory arises from the fractures which must often occur. If they happen with great frequency, it would seem as if the whole swarm of meteorites would degrade into dust. We know, however, that meteorites of consider-

* Added on November 16, 1888.

able size fall upon the earth, and, unless Mr. Lockyer has misinterpreted the spectroscopic evidence, the nebulæ do now consist of meteorites. Hence it would seem as if fracture was not of very frequent occurrence. It is easy to see that if two bodies meet with a given velocity the chance of fracture is much greater if they are large, and it is possible that the process of breaking up will go on only until a certain size, dependent on the velocity of agitation, is reached, and will then become comparatively unimportant.

When the volatilised gases cool they will condense into a metallic rain, and this may fuse with old meteorites whose surfaces are molten. A meteorite in that condition will certainly also pick up dust. Thus there are processes in action tending to counteract subdivision by fracture and volatilisation. The mean size of meteorites probably depends on the balance between these opposite tendencies. If this is so, there will be some fractures, and some fusions, but the mean mass will change very slowly with the mean kinetic energy of agitation. This view is at any rate adopted in the paper as a working hypothesis. It was not, however, possible to take account of fracture and fusion in the mathematical investigation, but the meteorites are treated as being of invariable mass.

The velocity with which the meteorites move is derived from their fall from a great distance towards a centre of aggregation. In other words, the potential energy of their mutual attraction when widely dispersed becomes converted, at least partially, into kinetic energy. When the condensation of a swarm is just beginning, the mass of the aggregation towards which the meteorites fall is small, and thus the new bodies arrive at the aggregation with small velocity. Hence initially the kinetic energy is small, and the volume of the sphere within which hydrostatic ideas are (if anywhere) applicable is also small. As more and more meteorites fall in, that volume is enlarged, and the velocity with which they reach the aggregation is increased. Finally the supply of meteorites in that part of space begins to fail, and the imperfect elasticity of the colliding bodies brings about a gradual contraction of the swarm. I do not now attempt to trace the whole history of a swarm, but the object of the paper is to examine its mechanical condition at an epoch when the supply of meteorites from outside has ceased, and when the velocities of agitation and distribution of meteorites in space have arranged themselves into a sub-permanent condition, only affected by secular changes. This examination will enable us to understand, at least roughly, the secular change as the swarm contracts, and will throw light on other questions.

The foundation for the mathematical investigation in the paper is the hypothesis that a number of meteorites which were ultimately to coalesce, so as to form the sun and planets, have fallen together from

a condition of wide dispersion, and form a swarm in which collisions are frequent.

For the sake of simplicity, the bodies are treated as spherical, and in the first instance as being of uniform size.

It is assumed provisionally that the kinetic theory of gases may be applied for the determination of the distribution of the meteorites in space. No account being taken of the rotation of the system, the meteorites will be arranged in concentric spherical layers of equal density of distribution, and the quasi-gas, whose molecules are meteorites, being compressible, the density will be greater towards the centre of the swarm. The elasticity of a gas depends on the kinetic energy of agitation of its molecules, and therefore in order to determine the law of density in the swarm we must know the distribution of kinetic energy of agitation.

It is assumed that when the system comes under our notice, uniformity of distribution of energy has been attained throughout a central sphere, which is surrounded by a layer of meteorites with that distribution of kinetic energy which, in a gas, corresponds to convective equilibrium, and with continuity of density and velocity of agitation at the sphere of separation. Since in a gas in convective equilibrium the law connecting pressure and density is that which holds when the gas is contained in a vessel impermeable to heat, such an arrangement of gas has been called by M. Ritter* an isothermal-adiabatic sphere, and the same term is adopted here as applicable to a swarm of meteorites. The justifiability of these assumptions will be considered later.

The first problem which presents itself then is the equilibrium of an isothermal sphere of gas under its own gravitation. The law of density is determined in the paper, but it will here suffice to remark that, if a given mass be enclosed in an envelope of given radius, there is a minimum temperature (or energy of agitation) at which isothermal equilibrium is possible. The minimum energy of agitation is found to be such that the mean square of velocity of the meteorites is almost exactly $\frac{2}{5}$ of the square of the velocity of a satellite grazing the surface of the sphere in a circular orbit.

As indicated above, it is supposed that in the meteor-swarm the rigid envelope, bounding the isothermal sphere, is replaced by a layer or atmosphere in convective equilibrium. The law of density in the adiabatic layer is determined in the paper, and it appears that when the isothermal sphere has minimum temperature, the mass of the adiabatic atmosphere is a minimum relatively to that of the isothermal sphere. Numerical calculation shows, in fact, that the isothermal sphere cannot amount in mass to more than 46 per cent. of the mass of the whole isothermal-adiabatic sphere, and that the limit of the

* 'Annalen der Physik und Chemie,' vol. 16 (1882), p. 166.

adiabatic atmosphere is at a distance equal to 2·786 times the radius of the isothermal sphere.*

It is also proved that the total energy, existing in the form of energy of agitation, is exactly one-half of the potential energy lost in the concentration of the matter from a condition of infinite dispersion. This result is brought about by a continual transfer of energy from a molar to a molecular form, for a portion of the kinetic energy of a meteorite is constantly being transferred into the form of thermal energy in the volatilised gases generated on collision. The thermal energy is then lost by radiation.

It is impossible as yet to sum up all the considerations which go to justify the assumption of the isothermal-adiabatic arrangement, but it is clear that uniformity of kinetic energy must be principally brought about by a process of diffusion. It is therefore interesting to consider what amount of inequality in the kinetic energy would have to be smoothed away.

The arrangement of density in the isothermal-adiabatic sphere being given, it is easy to compute what the kinetic energy would be at any part of the swarm, if each meteorite fell from infinity to the neighbourhood where we find it, and there retained all the velocity due to such fall. The variation of the square of this velocity gives an indication of the amount of kinetic energy which has to be degraded by conversion into heat and distributed by diffusion, in the attainment of uniformity. This may be called "the theoretical value of the kinetic energy." It appears that in the swarm, this square of velocity rises from zero at the centre of the swarm to a maximum, which is attained nearly half-way through the adiabatic layer, and then diminishes. It is found that the variations of this theoretical value are inconsiderable throughout the greater part of the range. Since this "theoretical value of the kinetic energy" is zero at the centre, there must be diffusion of kinetic energy from without inwards, and considerations of the same kind show that when a planet consolidates there must be a cooling of the middle strata both outwards and inwards.

We must now consider the nature of the criterion which determines whether the hydrostatic treatment of a meteor-swarm is permissible.

The hydrodynamical treatment of an ideal plenum of gas leads to the same result as the kinetic theory with regard to any phenomenon involving purely a mass, when that mass is a large multiple of the mass of a molecule; to any phenomenon involving purely a length, when the cube of that length contains a large number of molecules; and to any phenomenon involving purely a time, when that time is a large multiple of the mean interval between collisions. Again, any

* This is one of the results established by M. Ritter in a series of papers in the 'Annalen der Physik und Chemie' from 1878 onwards.

velocity to be justly deduced from hydrodynamical principles must be expressible as the edge of a cube containing many molecules passed over in a time containing many collisions of a single molecule; and a similar statement must hold of any other function of mass, length, and time.

Beyond these limits we must go back to the kinetic theory itself, and in using it care must be taken that enough molecules are considered at once to impart statistical constancy to their properties.

There are limits then to the hydrodynamical treatment of gases, and the like must hold of the parallel treatment of meteorites.

The principal question involved in the nebular hypothesis seems to be the stability of a rotating mass of gas; but unfortunately this has remained up to now an untouched field of mathematical research. We can only judge of probable results from the investigations which have been made concerning the stability of a rotating mass of liquid. Now it appears that the instability of a rotating mass of liquid first enters through the graver modes of gravitational oscillation. In the case of a rotating spheroid of revolution the gravest mode of oscillation is an elliptic deformation, and its period does not differ much from that of a satellite which revolves round the spheroid so as to graze its surface. Hence, assuming for the moment that a kinetic theory of liquids had been formulated, we should not be justified in applying the hydrodynamical method to this discussion of stability, unless the periodic time of such a satellite were a large multiple of the analogue of the mean free time of a molecule of liquid.

Carrying then this conclusion on to the kinetic theory of meteorites, it seems probable that hydrodynamical treatment must be inapplicable for the discussion of such a theory as the meteoric-nebular hypothesis, unless a similar relation holds good.

These considerations, although of a vague character, will afford a criterion of the applicability of hydrodynamics to the kind of problem suggested by the nebular hypothesis. And certain criteria suggested by this line of thought are found in the paper; they give a measure of the degree of curvature of the average path pursued by a meteorite between two collisions.

After these preliminary investigations, we have to consider what kind of meeting of two meteorites will amount to an "encounter" within the meaning of the kinetic theory.

Is it possible, in fact, that two meteorites can considerably bend their paths under the influence of gravitation, when they pass near one another? This question is considered in the paper, and it is shown that unless the bodies have the dimensions of small planets, the mutual gravitational influence is insensible. Hence, nothing short of absolute impact is to be considered an encounter in the kinetic theory,

and what is called the radius of "the sphere of action" is simply the distance between the centres of a pair when they graze, and is therefore the sum of the radii of a pair, or, if of uniform size, the diameter of one of them.

The next point to consider is the mass and size which must be attributed to the meteorites.

The few samples which have been found on the earth prove that no great error can be committed if the average density of a meteorite be taken as a little less than that of iron, and I accordingly suppose their density to be six times that of water.

Undoubtedly in a meteor-swarm all sizes co-exist (a supposition considered hereafter); for even if originally of uniform size they would, by subsequent fracture, be rendered diverse. But in the first consideration of the problem they have been treated as of uniform size, and as actual sizes are nearly unknown, results are given for meteorites weighing $3\frac{1}{8}$ grams. From these, the values for other masses are easily derivable.

It is known that meteorites are actually of irregular and angular shapes, but certainly no material error can be incurred when we treat them as being spheres.

The object of all these investigations is to apply the formulæ to a concrete example. The mass of the system is therefore taken as equal to that of the sun, and the limit of the swarm at any arbitrary distance from the present sun's centre. The theory is of course more severely tested the wider the dispersion of the swarm, and accordingly in a numerical example the outside limit of the solar swarm is taken at $44\frac{1}{2}$ times the earth's distance from the sun, or further beyond the planet Neptune than Saturn is from the sun. This assumption makes the limit of the isothermal sphere at a distance 16, about halfway between Saturn and Uranus.

In this case the mean velocity of the meteorites in the isothermal sphere is $5\frac{1}{3}$ kilometers per second, being $\sqrt{\frac{2}{3}}$ of the linear velocity of a planet revolving about a central body with a mass equal to 46 per cent. of that of the sun, at distance 16. In the adiabatic layer it diminishes to zero at distance $44\frac{1}{2}$. This velocity is independent of the size of the meteorites. The mean free path between collisions ranges from 42,000 kilometers at the centre, to 1,300,000 kilometers at radius 16, and to infinity at radius $44\frac{1}{2}$. The mean interval between collisions ranges from a tenth of a day at the centre, to three days at radius 16, and to infinity at radius $44\frac{1}{2}$. The criterion of applicability of hydrodynamics ranges from $\frac{1}{50000}$ at the distance of the asteriods to $\frac{1}{1300}$ at radius 16, and to infinity at radius $44\frac{1}{2}$.

All these quantities are ten times as great for meteorites of $3\frac{1}{8}$ kilos., and a hundred times as great for meteorites of $3\frac{1}{8}$ tonnes.

From a consideration of the tables in the paper it appears that,

with meteorites of $3\frac{1}{8}$ kilos., the collisions are sufficiently frequent even beyond the orbit of Neptune to allow the kinetic theory to be applicable in the sense explained. But if the meteorites weigh $3\frac{1}{8}$ tonnes, the criterion ceases to be very small at about distance 24, and if they weigh 3125 tonnes they cease to be very small at about the orbit of Jupiter. It may be concluded then that, as far as frequency of collision is concerned, the hydrodynamical treatment of a swarm of meteorites is justifiable.

Although the numerical results are necessarily affected by the conjectural values of the mass and density of the meteorites, yet it was impossible to arrive at any conclusion whatever as to the validity of the theory without numerical values, and such a discussion as the above was therefore necessary.

I now pass on to consider some results of this view of a swarm of meteorites, and to consider the justifiability of the assumption of an isothermal-adiabatic arrangement of density.

With regard to the uniformity of distribution of kinetic energy in the isothermal sphere, it is important to ask whether or not sufficient time can have elapsed in the history of the system to allow of the equalisation by diffusion.

It is shown therefore in the paper that in the case of the numerical example primitive inequalities of kinetic energy would, in a few thousand years, be sensibly equalised over a distance some ten times as great as our distance from the sun. This result then goes to show that we are justified in assuming an isothermal sphere as the centre of the swarm. As, however, the swarm contracts the rate of diffusion diminishes as the inverse $\frac{5}{2}$ power of its linear dimensions, whilst the rate of generation of inequalities of distribution of kinetic energy, through the imperfect elasticity of the meteorites, increases. Hence, in a late stage of the swarm, inequalities of kinetic energy would be set up, there would be a tendency to the production of convective currents, and thus the whole swarm would probably settle down to the condition of convective equilibrium throughout.

It may be conjectured then that the best hypothesis in the early stages of the swarm is the isothermal-adiabatic arrangement, and later an adiabatic sphere. It has not seemed worth while to discuss this latter hypothesis in detail at present.

The same investigation also gives the coefficient of viscosity of the quasi-gas, and shows that it is so great that the meteor-swarm must, if rotating, revolve nearly without relative motion of its parts, other than the motion of agitation. But as the viscosity diminishes when the swarm contracts, this would probably not be true in the later stages of its history, and the central portion would probably rotate more rapidly than the outside. It forms, however, no part of the scope of this paper to consider the rotation of the system.

The rate of loss of kinetic energy through imperfect elasticity is next considered, and it appears that the rate, estimated per unit time and volume, must vary directly as the square of the quasi-pressure, and inversely as the mean velocity of agitation. Since the kinetic energy lost is taken up in volatilising solid matter, it follows that the heat generated must follow the same law. The mean temperature of the gases generated in any part of the swarm depends on a great variety of circumstances, but it seems probable that its variation would be according to some law of the same kind. Thus, if the spectroscope enables us to form an idea of the temperature in various parts of a nebula, we shall at the same time obtain some idea of the distribution of density.

It has been assumed that the outer portion of the swarm is in convective equilibrium, and therefore there is a definite limit beyond which it cannot extend. Now a medium can only be said to be in convective equilibrium when it obeys the laws of gases, and the applicability of those laws depends on the frequency of collisions. But at the boundary of the adiabatic layer the velocity of agitation vanishes, and collisions become infinitely rare. These two propositions are mutually destructive of one another, and it is impossible to push the conception of convective equilibrium to its logical conclusion. There must, in fact, be some degree of rarity of density and of collisions at which the statistical treatment of the medium breaks down.

I have sought to obtain some representation of the state of things by supposing that collisions never occur beyond a certain distance from the centre of the swarm.

Then from every point of the surface of the sphere, which limits the region of collisions, a fountain of meteorites is shot out, in all azimuths and at all inclinations to the vertical, and with velocities grouped about a mean according to the law of error. These meteorites ascend to various heights, without collision, and, in falling back on to the limiting sphere, cannonade its surface, so as to counterbalance the hydrostatic pressure at the limiting sphere.

The distribution in space of the meteorites thus shot out is investigated in the paper, and it is found that near the limiting sphere the decrease in density is somewhat more rapid than the decrease corresponding to convective equilibrium.

But at more remote distances the decrease is less rapid, and the density ultimately tends to vary inversely as the square of the distance from the centre.

It is clear that according to this hypothesis the mass of the system is infinite in a mathematical sense; for the existence of meteorites with nearly parabolic and hyperbolic orbits necessitates an infinite number, if the loss of the system shall be made good by the supply.

But if we consider the subject from a physical point of view, this conclusion appears unobjectionable.* The ejection of molecules with exceptionally high velocities from the surface of a liquid is called evaporation, and the absorption of others is called condensation. The general history of a swarm, as sketched at the beginning, may then be put in different words, for we may say that at first a swarm gains by condensation, that condensation and evaporation balance, and finally that evaporation gains the day.

If the hypothesis of convective equilibrium be pushed to its logical conclusion, we reach a definite limit to the swarm, whereas if collisions be entirely annulled the density goes on decreasing inversely as the square of the distance. The truth must clearly lie between these two hypotheses. It is thus certain that even the small amount of evaporation, shown by the formulæ derived from the hypothesis of no collision, must be in excess of the truth; and it may be that there are enough waifs and strays in space ejected from other systems to make good loss. Whether or not the compensation is perfect, a swarm of meteorites would pursue its evolution without being sensibly affected by a slow evaporation.

Up to this point the meteorites have been considered as of uniform size, but it will be well to examine the more truthful hypothesis that they are of all sizes, grouped about a mean according to a law of

It appears, from the investigation in the paper, that the larger stones move slower, the smaller ones faster, and the law is that the mean kinetic energy is the same for all sizes. It is proved that the mean path between collisions is shorter in the proportion of 7 to 11, and the mean frequency of collision greater in the proportion of 4 to 3, than if the meteorites were of uniform mass equal to the mean. Hence the numerical results found for meteorites of uniform size are applicable to non-uniform meteorites of a mean mass about a quarter greater than the uniform mass; for example, the results for uniform meteorites of $3\frac{1}{4}$ tonnes apply to non-uniform ones of mean mass a little over 4 tonnes.

The means here spoken of refer to all sizes grouped together, but there is a separate mean free path and mean frequency appropriate to each size. These are investigated in the paper, and their values illustrated in a figure. It appears that collisions become infinitely frequent for the infinitely small ones, because of their infinite velocity, and again infinitely frequent for the infinitely large ones, because of their infinite size. There is a minimum frequency of collision for a

* [It must be borne in mind that the very high velocities which occur occasionally in a medium with perfectly elastic molecules, must happen with great rarity amongst meteorites. An impact of such violence that it *ought* to generate a hyperbolic velocity will probably merely cause fracture.—Added November 23, 1888.]

certain size, a little less in radius than the mean radius, and considerably less in mass than the mean mass.

For infinitely small meteorites the mean free path reaches a finite limit, equal to about four times the grand mean free path; and for infinitely large ones, the mean free path becomes infinitely short. It must be borne in mind that there are infinitely few of the infinitely large and infinitely small meteorites. Variety of size does not then, so far, materially affect the results.

But a difference arises when we come to consider the different parts of the swarm. The larger meteorites, moving with smaller velocities, form a quasi-gas of less elasticity than do the smaller ones. Hence the larger meteorites are more condensed towards the centre than are the smaller ones, or the large ones have a tendency to fall down, whilst the small ones have a tendency to rise. Accordingly, the various kinds are to some extent sorted according to size.

An investigation is made in the paper of the mean mass of meteorites at various distances from the centre, both inside and outside of the isothermal sphere, and a figure illustrates the law of diminution of mean mass.

It is also clear that the loss of the system through evaporation must fall more heavily on the small meteorites than on the large ones.

After the foregoing summary, it will be well to briefly recapitulate the principal physical conclusions which seem to be legitimately deducible from the whole investigation; in this recapitulation qualifications must necessarily be omitted or stated with great brevity.

When two meteorites are in collision, they are virtually highly elastic, although ordinary elasticity must be nearly inoperative.

A swarm of meteorites is analogous with a gas, and the laws governing gases may be applied to the discussion of its mechanical properties. This is true of the swarm, from which the sun was formed, when it extended beyond the orbit of the planet Neptune.

When the swarm was very widely dispersed the arrangement of density and of velocity of agitation of the meteorites was that of an isothermal-adiabatic sphere. Later in its history, when the swarm had contracted, it was probably throughout in convective equilibrium.

The actual mean velocity of the meteorites is determinable in a swarm of given mass, when expanded to a given extent.

The total energy of agitation in an isothermal-adiabatic sphere is half the potential energy lost in the concentration from a condition of infinite dispersion.

The half of the potential energy lost, which does not reappear as kinetic energy of agitation, is expended in volatilising solid matter, and heating the gases produced on the impact of meteorites. The heat so generated is gradually lost by radiation.

The amount of heat generated per unit time and volume varies as

the square of the quasi-hydrostatic pressure, and inversely as the mean velocity of agitation. The temperature of the gases volatilised probably varies by some law of the same nature.

The path of a meteorite is approximately straight, except when abruptly deflected by a collision with another. This ceases to be true at the outskirts of the swarm, where the collisions have become rare. The meteorites here describe orbits under gravity which are approximately elliptic, parabolic, and hyperbolic.

In this fringe to the swarm the distribution of density ceases to be that of a gas under gravity; and as we recede from the centre the density at first decreases more rapidly, and afterwards less rapidly than if the medium were a gas.

Throughout all the stages of its history there is a sort of evaporation by which the swarm very slowly loses in mass, but this loss is more or less counterbalanced by condensation. In the early stages the gain by condensation outbalances the loss by evaporation, they then equilibrate, and finally the evaporation may be greater than condensation.

Throughout the swarm the meteorites are to some extent sorted according to size; as we recede from the centre the number of small ones preponderates more and more, and thus the mean mass continually diminishes with increasing distance. The loss by evaporation falls principally on the small meteorites.

A meteor swarm is subject to gaseous viscosity, which is greater the more widely diffused is the swarm. In consequence of this a widely extended swarm, if in rotation, will revolve like a rigid body without relative motion (other than agitation) of its parts.

Later in the history the viscosity will probably not suffice to secure uniformity of rotation, and the central portion will revolve more rapidly than the outside.

[The kinetic theory of meteorites may be held to present a fair approximation to the truth in the earlier stages of the evolution of the system. But later the majority of the meteors must have been absorbed by the central sun and its attendant planets, and amongst the meteors which remain free the relative motion of agitation must have been largely diminished. These free meteorites—the dust and refuse of the system—probably move in clouds, but with so little remaining motion of agitation that (except perhaps near the perihelion of very eccentric orbits) it would scarcely be permissible to treat the cloud as in any respect possessing the mechanical properties of a gas.]*

The value of this whole investigation will appear very different to different minds. To some it will stand condemned as altogether too speculative, others may think that it is better to risk error in the

* Added November 22, 1888.

chance of winning truth. To me at least it appears that the line of thought flows in a true channel, that it may help to give a meaning to the observations of the spectroscopist, and that many interesting problems, here barely alluded to, may perhaps be solved with sufficient completeness to throw light on the evolution of nebulæ and planetary systems.

III. "On the Secretion of Saliva, chiefly on the Secretion of Salts in it." By J. N. Langley, M.A., F.R.S., Fellow of Trinity College, and H. M. Fletcher, B.A., Trinity College, Cambridge. Received August 17, 1888.

(Abstract.)

Heidenhain has shown that when saliva is obtained by stimulating the chorda tympani, the percentage of salts in the saliva depends upon the rate of secretion, so that the faster the secretion the higher the percentage of salts is up to a limit of about 0·6 per cent. Werther has come to the same conclusion, but finds that the percentage of salts may be as much as 0·77. Both in Heidenhain's and in Werther's experiments there are many exceptions to this rule, attributed by them to variations in the rate of secretion of saliva during the time of collecting any one sample.

We have repeated, with some modifications, the experiments of Heidenhain, paying especial attention to the rate of secretion of saliva, and find in 10 out of 11 cases, that his law of an increase in the percentage of salts with an increase in the rate of secretion holds. The single exception may be due to a modification of the blood-flow through the gland during the time of collecting the saliva. The slowly secreted saliva contains a low percentage of salts, whether it is produced by a weak nerve stimulus, or by a very strong nerve stimulus which lowers the irritability of the nerve-fibres.

We do not find any rate of secretion, beyond which an increase in rate fails to increase the percentage of salts in the saliva. The increment in the percentage of salts decreases, however, with each equal successive increment in the rate of secretion.

As a rule in saliva obtained by injecting pilocarpin, the percentage of salts follows Heidenhain's law; we take the exceptions to be due to the action of pilocarpin upon the circulation, the blood-flow through the gland being less than normally accompanies the degree of stimulation of the gland cells.

The percentage of salts in saliva obtained by stimulating the sympathetic is higher than corresponds to its rate of secretion, the saliva obtained by stimulating the chorda being taken as a basis of comparison; this sympathetic saliva may be secreted at $\frac{1}{150}$th of the rate

of chorda saliva, and yet contain very nearly as high a percentage of salts.

Dyspnœa decreases the rate of secretion of saliva with a given stimulus, and if not too prolonged, increases the percentage of salts, and tends to increase the percentage of organic substance in the saliva. This holds whether the saliva be obtained by stimulating the chorda tympani, or by injecting pilocarpin. Dyspnœa has, for a short time, an after-action, tending also to increase the percentage of salts, and possibly that of organic substance.

Clamping the carotid during secretion has the same general effect as dyspnœa, but it causes a still more marked increase in the percentage of salts. Its after-effect is also much greater, and lasts longer.

Bleeding has a similar effect to dyspnœa and to clamping the carotid, but its most marked effect is an increase in the percentage of organic substance.

Injection of dilute salt solution, NaCl, 0·2 to 0·6 per cent., in sufficient quantity, considerably increases the rate of secretion of saliva; the percentage of salts in the saliva decreases, although the rate of secretion of salts usually increases; the percentage of organic substance decreases; that is, increasing the volume of the blood with dilute salt solution chiefly increases the rate of secretion of water.

The percentage of salts in samples of saliva obtained *after* the injection of dilute salt solution, increases with the rate of secretion, it is only when these are obtained before the injection that a discrepancy in the normal relation between percentage of salts and rate of secretion of water appears.

Injection of sodium carbonate 2 per cent. also increases the rate of secretion of saliva; in this case the percentage of salts is about normal, the percentage of organic substance falls slightly only, *i.e.*, the irritability either of the nerve-fibres or of the gland cells is increased.

Injection of considerable doses of potassium iodide, 1 per cent., after the sodium carbonate still allows a rapid secretion, but the percentage of salts falls.

Injection of strong salt solution increases the percentage of salts in saliva, this is in accordance with the recent observations of Novi that the chlorine in the salts of saliva is increased for a given rate of secretion by increasing the percentage of sodium chloride in the blood. We find, however, that in the case of an injection of strong salt solution into the blood which leaves the secretory power of the gland unaffected, the increase in the percentage of salts is much greater with slowly than with rapidly secreted saliva, and that when the secretory power of the gland is affected by strong salt solution, an increase in the percentage of organic substance also takes place; this and a part of the increase in the percentage of salts we attribute to a decrease of the blood-flow through the gland.

Saliva produced by stimulating the chorda tympani, or by injecting pilocarpin, after a small dose of atropin has been given, contains a low percentage of organic substance and of salts.

We, like Werther, find that sub-lingual saliva has a considerably higher percentage of salts than sub-maxillary saliva.

If lithium citrate, potassium iodide, potassium ferrocyanide, and pilocarpin are injected into the blood, lithium can be detected in the first drops of saliva secreted, potassium iodide after the first six drops; potassium ferrocyanide cannot be detected at any stage of secretion.

The general result of these experiments is to show that the secretion of water, of salts, and of organic substance are differently affected by different conditions, and that the percentage composition of saliva is determined by the strength of the stimulus, by the character of the blood, and by the amount of blood supplied to the gland.

All or nearly all the arguments which have been adduced to prove that the secretion of organic substance is governed by special nerve-fibres, have their counterparts with regard to the secretion of salts, so that we might imagine at least three kinds of secretory fibres to be present. The experiments, on the whole, indicate that this complicated arrangement does not exist, but that the stimulation of a single kind of nerve-fibre produces varying effects according to the varying conditions of the gland cells.

IV. "Observations upon the Electromotive Changes in the Mammalian Spinal Cord following Electrical Excitation of the Cortex Cerebri. Preliminary Notice." By FRANCIS GOTCH, Hon. M.A. Oxon, B.A., B.Sc. Lond., and VICTOR HORSLEY, B.S., F.R.S., Professor of Pathology, University College, London. (From the Physiological Laboratory of the University of Oxford.) Received August 27, 1888.

[PLATE 1.]

Hitherto pathologists have attempted the analysis of the epileptic convulsion by the graphic method, that is, by recording the spasmodic contractions of the muscles involved. Recent investigations of this kind have shown that the excitation of the cortex cerebri, whether by electrical or chemical means, or by the presence of certain pathological states, neoplasms, inflammation, &c., is invariably followed in the higher mammals by a definite and characteristic sequence of movements in the muscles. It is, however, obvious that such investigations have up to the present succeeded in determining the characters of the neural disturbance only when this has reached the peripheral

terminations of the efferent nerves. Now since the excitatory processes originating in the cortex are conducted by the efferent channels in the spinal cord, presumably the pyramidal tracts, the problem of their relationship to the centres of the bulbo-spinal system cannot be determined by experiments which record the mechanical changes in the muscles. In order to ascertain what share respectively the centres in the cortex and those in the spinal cord have in the production of the characteristic epileptic sequence, the action of the latter must be eliminated. This can be done by investigating the nature of the excitatory processes in the cord when the efferent channels in the dorsal region for the lower limbs are made the subject for observation.

For this purpose we determined to obtain, if possible, evidence as to the nature of the excitatory processes of the epileptic convulsion in the spinal cord, as shown by "tapping" the cord and noting the electromotive changes which, as is well known, accompany functional activity in nerves. The results we have already obtained are so harmonious and demonstrative, that we venture to make this preliminary communication, reserving full details for a subsequent account.

PART I. *The Electromotive Change following a Single Excitation of the Mammalian Nerve.*

Our first experiments were made for the purpose of ascertaining to what extent we could detect an electromotive change following a single excitation of a mammalian nerve. Since the discovery by du Bois-Reymond of the fact that the excitatory process in nerve is accompanied by an electromotive change, the characters and time relations of this change have been investigated by various observers, notably by Bernstein, Hermann, Hering, and Head. The general result of their observations is to show that the change following a single stimulus is of very short duration, so short that the galvanometer gives little evidence of its presence, and the observers referred to were compelled to adopt the device first employed by Bernstein, which involves repeated excitation and consequent summation of effect, a method well known to physiologists as that of the repeating differential rheotome. For our purpose it was essential to obtain evidence of the effect following one stimulus only, and this we were fortunately able to do by using a sensitive Lippmann's capillary electrometer of quick reaction, made by Mr. G. F. Burch, and belonging to Dr. Burdon Sanderson, who kindly placed it at our disposal. This instrument, when the capillary was magnified 400 times by the observing microscope, gave a perceptible response when connected through a resistance of 10,000 ohms for one-thousandth of a second with an electromotive difference of only 0·003 D. The amount of movement of the mercury was estimated by the divisions of a micrometer eyepiece, one

division of which indicated an actual movement of $\frac{1}{250}$ of a millimetre. After we had found that the electrometer, when connected with the transverse and longitudinal surfaces of the sciatic nerve of the toad, showed a response of one division following the application of a single stimulus, whether electrical or mechanical, we proceeded *to the examination of the sciatic nerve in the rabbit, cat, and monkey*. For these experiments the animal was in every case kept under the influence of ether, which was maintained throughout the whole experiment, and the animal was killed before recovery. The sciatic nerve seemed for many reasons the most suitable of the mammalian nerves. It can be quickly prepared for 7 or 8 cm. in length; its nutrition is well preserved, since the *arteria comes nervi ischiadici* can be left uninjured, and its diameter lessens the dangers of drying.

The nerve, having been rapidly prepared and bathed in warm saline solution, 0·6 per cent., was ligatured low down in the thigh, the ligature including the popliteal trunks. It was then divided on the peripheral side of the knot, and raised in air so as to be at right angles to the limb. One kaolin pad of a non-polarisable electrode was applied to the cut end, and another to the longitudinal surface at a distance of 1·5 cm. A pair of sheathed exciting platinum electrodes 2 mm. apart, was then applied to the trunk of the nerve 6 cm. centrally from the nearest leading-off electrode, *i.e.*, opposite the sciatic notch. The exciting stimulus was obtained by the break of the current of a single Callaud cell supplying the primary coil of a du Bois-Reymond inductorium graduated by Kronecker. The break shock produced in the secondary coil by this means was so feeble as to be barely perceptible on the tip of the tongue when the secondary coil completely covered the primary. The break was effected by the spring rheotome, which opened a fixed key at a definite point in its course. The electrometer was connected with the non-polarisable electrodes by a circuit which included the usual compensator. By means of a switch the electrometer could be cut out, and the circuit made to include a high resistance galvanometer, which also revealed the single variation. The two instruments could be thus readily compared. The excursion of the mercury of the electrometer was ascertained both by direct observation in terms of the divisions of the micrometer eyepiece, and by photographing the projected capillary upon a moving sensitive plate; in the latter case the capillary was magnified 100 times. The results of our observations are briefly as follows:—

The mammalian nerve showed a well-marked difference or demarcation current, that is to say, the electrode upon the longitudinal surface was notably positive to that on the cut end. The movement of the mercury corresponding to this difference amounted in some cases to 60 divisions of the micrometer, and is shown in fig. 1 pro-

jected upon the plate. Its E.M.F. was from about 0·01 to 0·015 D. The passage of the single break induction shock through the platinum electrodes in either direction was followed by a small quick movement of the mercury, which was invariably in the opposite direction to that produced by the demarcation current. Its amount varied in different animals from 1 to 2·5 divisions of the micrometer eyepiece, and it is shown as photographed in fig. 1 and fig. 2. After severing the nerve from the bulbo-spinal system above the exciting electrodes, the same effect was obtained; its character, as shown by the movement of the mercury was, however, different, being as we believe much shorter in duration and less in amount. But our experiments not being directed to the elucidation of this point, we will not speak positively with regard to it. After a time, varying in different cases from twenty minutes to three-quarters of an hour, the effect was no longer visible. This movement of the mercury may be conceivably due to the three following factors, working singly or in co-operation:—

(A.) Escape of the exciting induction current (uni-polar).

(B.) Electrotonic change.

(C.) The true excitatory variation of the nerve.

(A.) That it was not due to any escape of the induction current is shown by the following facts:—

(1.) The variation was produced by the very weak induction currents, such as those obtained when the Helmholtz wire is used, and its character did not vary with increasing strength of the current.

(2.) It was no longer perceptible when the nerve was ligatured between the exciting and leading-off electrodes.

(3.) As the nerve gradually died the effect became less, and was no longer perceptible when the nerve was severed from the animal and left for three-quarters of an hour. Moreover, when the nerve was indifferently prepared the variation was absent, or else very small and transient.

(4.) The effect remained visible when the electrometer was short circuited for $\frac{1}{1000}$ second after the break of the exciting key.

(B.) That it was not due to electrotonic change is shown by the following additional facts:—

(1.) The direction of the effect was always the same, that is, opposed to that of the demarcation current whatever the direction of the exciting current.

(2.) When the exciting electrodes were shifted to within a centimetre of the proximal leading-off electrode, an effect was produced, the direction of which was dependent upon that of the exciting current (fig. 3). This effect differed from that of the true variation in other particulars, viz., its amount was dependent upon that of the

exciting current, it could be obtained after ligature of the nerve, and when thus obtained its character, as shown by the movement of the electrometer, was unlike that of the excitatory variation, both to the eye and in the photograph (compare figs. 1, 2, 3, Plate 1).

(3.) An excursion similar to that we are considering could be produced by mechanical excitation.

There is thus no doubt that the movement we obtained and photographed was due to the electromotive change which accompanies the propagation of an excitatory state along the mammalian nerve when this state is evoked by the application of a single stimulus.

Having thus assured ourselves of the accuracy of the method, we now proceeded to ascertain whether the instrument would reveal the existence of similar electromotive changes if it was connected with the nerve or with the spinal cord, and an epileptic convulsion produced by excitation of the cortex cerebri.

Part II. *Excitation of the Cortex Cerebri.*

A. *Mixed Spinal Nerve connected with the Electrometer.*—In two cases we have connected in the manner described in Part I the sciatic nerve with the electrometer, and have then exposed by a small trephine opening the so-called motor cortical centre for the lower limb. This we then excited by a very weak but adequate faradic current. So far, however, we have not been able to detect any movement in the mercury, although the muscles of the investigated limb supplied by the anterior crural nerve were thrown into a state of active convulsion. It is probable that the character of the neural disturbances in the mixed nerve may be best studied by investigations which we shall shortly undertake upon the electromotive changes in the muscles.

B. *The Spinal Cord connected with the Electrometer.*—The experiments, the results of which are now to be briefly detailed, were made in the following manner:—

The spinal cord of the etherised animal (cat and monkey) was exposed in the lower dorsal region for about 4 cm., and as low down as the upper end of the lumbar enlargement. Great care was taken by bathing with warm saline to guard as much as possible against the dangers of error due to cooling and drying. The dura mater having been split longitudinally, a strong thread was passed round the spinal cord at the lower limit of the part exposed. It was tied firmly and the cord divided below the knot. By successive division of the two or three roots exposed in the intervertebral foramina, the cord was easily raised from the neural canal and suspended in the air without any great interference with the circulation in the longitudinal vessels.

One of the non-polarisable electrodes was then brought into contact with the cut end of the cord and the knotted ligature, while the other was connected with the longitudinal surface of the cord 2 cm. from the cut end by means of soft thread cables soaked in saline solution and tied loosely round the cord. In one experiment the connexion was with one lateral column only. Mass movements of the electrodes upon the spinal cord were suitably guarded against, though it was found that the cord might be shaken without producing any effect in the electrometer.

On connecting these electrodes with the electrometer a considerable electromotive difference was found to exist between the contacts, the excursion of the mercury being so great, *i.e.*, beyond the field of the microscope, that its amount could not be estimated in terms of the micrometer eyepiece. The cut surface was always negative to the longitudinal surface, and the amount of the difference as estimated by the compensation method was about 0·02 D. It appeared to be highest when the section passed through the dorsal region without involving the lumbar enlargement. A difference between the surfaces of the cord has been previously observed by du Bois-Reymond.

The cortex cerebri was now exposed and the exciting circuit prepared. The inductorium previously employed was again used with one Daniell cell in connexion with the interrupter of primary coil and the Helmholtz side wire. The exciting electrodes had platinum points 2 mm. apart.

The demarcation current having been compensated, and the electrometer placed in connexion with the non-polarisable electrodes, the motor area for the lower limb was excited. The results of the observations made upon four monkeys and several cats may be summed up as follows:—

(1.) The application of the exciting electrodes to the cortex was without exception only followed by a movement in the electrometer when the area of representation of the lower limb was touched, and this even when owing to prolonged excitation of the arm area the upper limb was in violent epileptic convulsion. We found that when the exciting electrodes were moved over the surface of the brain the observer at the electrometer only gave notice of a movement in the instrument when the person exciting had crossed the margin of representation of the limbs. This shows that electromotive changes in the cord sufficient to affect our instrument occurred only when the motor area of the lower limb was excited. All error due to escape is thus set on one side, while at the same time this remarkable fact confirms the localisation of function.

(2.) The excitation of the motor area for the lower limb was accompanied and followed by characteristic movements of the mercury (figs. 4 and 5). The excitation by means of the interrupted

current usually lasted for two seconds, that is about 200 equal and alternately directed induction currents passed through the excited tissue. During this period the mercury showed an excursion opposed in direction to that of the difference between the longitudinal surface and cut end of the cord. This excursion persisted as long as the excitation lasted, and ceased when this was left off. Then after an interval of from one to three seconds there ensued a rhythmical succession of excursions each opposed in direction to the resting difference, some apparently single and others multiple. These lasted from twenty to thirty seconds and suddenly ceased.

The excursions varied in amount from one to about four divisions of the micrometer eyepiece, and their rate of occurrence was too rapid to be correctly estimated by the eye. We therefore obtained photographs of this rhythmical effect, and of these we append two (see figs. 4 and 5). The first of these (fig. 4) shows the electromotive change occurring in the spinal cord during a complete convulsion, in which may be distinguished the first persistent stage parallel to the tonic stage of the muscular epileptic convulsion and the second rhythmical series parallel to the clonic stage.

They are both shown upon the plate, which in this instance took about twelve seconds in travelling past the image of the capillary.

The second photograph (fig. 5), taken on a quickly travelling plate, shows the rhythmical stage only. The rate of the rhythm is seen to vary, and the individual variations to become more pronounced as the rhythm slows, that is, towards the end of the fit.

We have repeated this observation thirty or forty times, and feel ourselves justified in concluding that we have obtained evidence that during a cortical epileptiform discharge the electromotive changes in the spinal cord are exactly parallel as regards the character of their sequence to the convulsions of the muscles as recorded by the graphic method. It remains to be stated that after removal of the cortex we have obtained an effect in the electrometer when the corona radiata was stimulated. This effect was only present during the period of excitation, no rhythmical after-effect ever being observed. Its character was prolonged, and resembled the persistent stage referred to above (see fig. 6).

In conclusion, we consider that since by the method we have adopted the influence of the lumbar bulbo-spinal centres is excluded, the existence of the epileptic rhythm in the dorsal regions of the spinal cord points to its being almost entirely of cortical origin.

EXPLANATION OF PLATE 1.

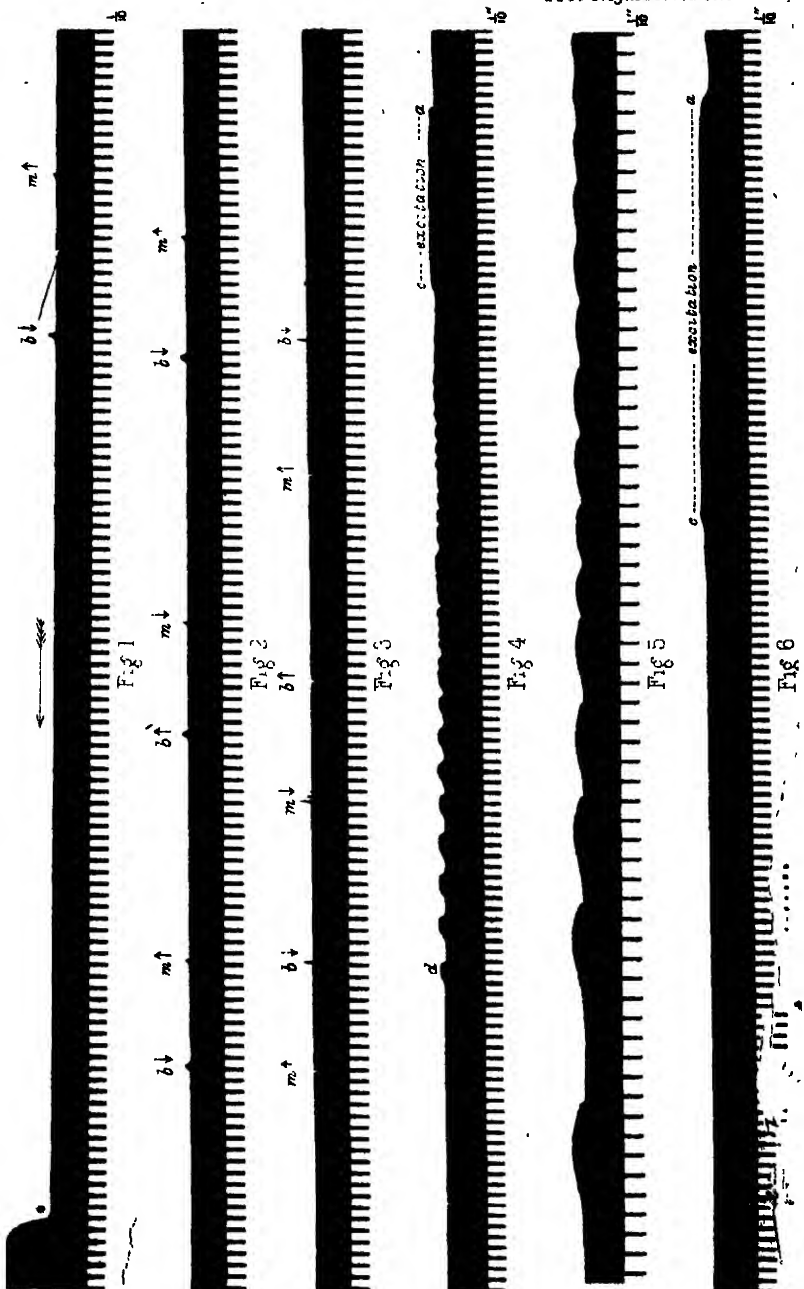
The six figures in the plate are facsimile drawings of photographs. The negatives were obtained by projecting the image of the capillary electrometer upon a narrow slit, behind which an extra rapid photographic plate travelled. The direction of movement was such that the right hand side of the prints corresponds to the moment when the plate reached the slit: the figures are thus to be read from right to left. In order to save room, only the essential part of the photographs—that showing the position of the meniscus of the mercury in the photograph—is shown. The lower darkly toned part of each figure corresponds to the lighter part of the negative, and indicates the part of the slit shaded by the mercury of the electrometer; an excursion of the mercury is thus indicated by an elevation or depression of the upper edge of the dark band. The regular series of dark and light bars on the edge of the figures were made by a vibrating shutter, each entire vibration of which occupied one-tenth of a second.

FIG. 1.—Photograph showing two prominences, *m* and *b*, due to two excursions of the mercury when first a make and then a break induction shock was led through the mammalian nerve, the cut end and surface of which were in connexion with the electrometer 6 cm. from the point of excitation. The arrows indicate the direction of the exciting induction current through the nerve, and the effect is seen to be independent of this direction. At the point marked * the electrometer was short circuited, and the movement of the mercury due to the cessation of the demarcation current effect is thus shown. The excursions at *m* and *b* are seen to be opposed in direction to that produced by the demarcation current.

FIG. 2.—Photograph showing the excitatory variation effect in nerve. In this case the nerve of the monkey was severed from the body, connected as in fig. 1 with the electrometer, and excited six times by means of induction shocks of different character and direction. The excitation occurred at make *m* and break *b*, and the direction of the induction shock—whether ↑, ascending, or ↓, descending—is indicated. The effect is seen to be always in the same direction, being opposed to that of the demarcation current, and such that the electrode on the longitudinal surface becomes negative to that on the cut section. The rate of movement of plate was the same as in fig. 1.

FIG. 3.—Photograph illustrating the effect produced in the electrometer when there is a slight escape from the exciting electrodes into the electrometer electrodes. The effect was produced by using a severed nerve, which no longer gave any obvious excitatory response to electrical excitation. The exciting electrodes were placed upon such a nerve at a very short distance (1·5 cm.) from the nearest leading off electrode, viz., that upon the longitudinal surface. The direction of the effect is seen to depend upon the direction of the induction shock as produced by make *m* and break *b* of the primary circuit of the induction apparatus. The character of the excursion is markedly different to that shown in figs. 1 and 2, being much more abrupt. The rate of movement of plate was the same as in fig. 1.

FIG. 4.—Photograph showing the effect produced in the electrometer when this is connected by one pole with the longitudinal, and by the other with the sectional, surface of the spinal cord of the monkey, and the *cortex cerebri* then excited over the motor area for the lower limbs by means of the faradic current. The excitation commenced at *a* and ceased at *c*. It is seen to be accompanied by an upward movement of the mercury, shown by an alteration in the position of the dark band, which reaches a slightly



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higher level and remains at this level during the period of excitation, and then returns. The direction of the movement indicates that the longitudinal surface has become negative to the cross-section. This corresponds to the persistent (tonic) muscular effect which is characteristic of the first stage of an epileptic fit. Proceeding from right to left, the cessation of the excitation is seen to be followed by a rhythmical series of excursions, which at first follow one another in rapid succession, but are small in extent, and which subsequently occur at longer intervals, but are much more pronounced in character, until at *d* they suddenly cease. This corresponds to the clonic stage of the epileptic convulsion.

FIG. 5.—The photograph shows the rhythmical (clonic) effect only. The recording surface was made to travel more rapidly past the slit, a marked rhythmical change having been first evoked by excitation of the cortex. The plate was not allowed to commence its passage past the slit until six seconds after the excitation had ceased. The rhythm is thus seen to great advantage. As before, the upward movement of the mercury, as indicated by the elevations of the more darkly toned parts, are due to electromotive changes in the cord such that the longitudinal surface of the cord becomes negative to the transverse section.

FIG. 6.—Photograph showing the effect obtained when, with the spinal cord connected as in the preceding with the electrometer, the *cortex cerebri* is removed and the *corona radiata* excited by faradisation. The excitation commenced at *a* and ceased at *c*. It is accompanied by an upward persistent movement of the mercury, shown in the photograph as an alteration of level, and corresponding in character to the (tonic) effect produced during the excitation of the cortex. On the cessation of the stimulus the effect subsides and is *not* followed by any rhythmical effect.

Presents, November 15, 1888.

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November 22, 1888.

Professor G. G. STOKES, D.C.L., President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council nominated for election was read as follows:—

President.—Professor George Gabriel Stokes, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.—{ Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.

Other Members of the Council.—Professor Henry Edward Armstrong, Ph.D.; Henry Bowman Brady, F.G.S.; Charles Baron Clarke, M.A.; William Huggins, D.C.L.; John Whitaker Hulke, F.R.C.S.; Professor John W. Judd, F.G.S.; Edward Emanuel Klein, M.D.; Professor E. Ray Lankester, M.A.; Professor Herbert McLeod, F.I.C.; Sir James Paget, Bart., D.C.L.; William Pole, Mus. Doc.; William Henry Preece, M.I.C.E.; Sir Henry E. Roscoe, D.C.L.; Edward John Routh, D.Sc.; Professor Arthur William Rücker, M.A.; William James Lloyd Wharton, Capt. R.N.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "On the Specific Heats of Gases at Constant Volume. (Preliminary Note.)" By J. JOLY, M.A., B.E. Communicated by Professor G. F. FITZGERALD, F.R.S. Received July 21, 1888.

I have found it possible to obtain the specific heat of a gas at constant volume by means of the steam calorimeter,* the values obtained being, I believe, reliable as close approximations to the true values.

* "On the Method of Condensation in Calorimetry" (by J. Joly) 'Roy. Soc. Proc.,' vol. 41, p. 352; and "Ueber das Dampfcalorimeter" (von R. Bunsen), 'Wiedemann's Annalen,' vol. 31, p. 1.

The first method of procedure adopted was to compress by means of a pump a certain quantity of dry air into a thin copper sphere, the sphere being then closed by a screw valve. The quantity of gas in the sphere is ascertained by weighing.

The sphere is now hung in the calorimeter, suspended from a delicate balance, reading to one-tenth of a milligram, and its thermal capacity determined in a certain number of experiments. The gas is then released, and the sphere further exhausted by means of an air-pump, sealed, and its thermal capacity again determined in a number of experiments. This allows of a computation of the thermal capacity of the contained gas.

This method I at first used, but as in dealing with the effect on the weighings, due to the transference of so bulky a body from air to steam, much troublesome calculation and risk of error was involved, I modified it in the following manner:—

Two spheres are prepared, alike with respect to external volume, and approximately of the same weight. The thermal capacities of these are compared in a double calorimeter, being suspended one from each arm of a short beam balance. If their thermal capacities are not alike a calculated weight of copper wire is introduced into that of least thermal capacity. They are in this way brought to have the same thermal capacity, so that in an experiment on the empty spheres there is no effect on the balance.

One of these is now pumped full of air, and the specific heats of the spheres again compared. The weight of condensation now obtained is that due to the gas alone. It is evident that many sources of error obtaining in the former method are removed in the latter. The results obtained are also far more consistent one with another. In this case the specific heat is calculated directly on the formula given in my paper on the steam calorimeter—

$$S = \frac{w\lambda}{W(t_2 - t_1)},$$

where λ is the latent heat of steam, w the weight of steam condensed by the gas, W the weight of gas, and t_2 t_1 the extremes of temperature obtaining. S so calculated may be subject to some slight corrections, which I will not here enter into.

Up to the present I have only dealt with air, but I have made preparations for resuming shortly my work, dealing with other gases, over critical temperatures if possible in some cases, and making confirmatory experiments on air and also in extension of those given below.

The spheres used are about 6·7 cm. internal diameter; volume 158·5 c.c. They weigh about 92·2 grams. That containing the air is tested hydraulically to 1000 lbs. per square inch.

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P. 34, line 7. After the word *balance*, reference to Note on p. 36 *omitted*.

Table I.

Weight of Air in the Sphere = 5·4816 grammes.
Pressure at 100° C. about 27,700 mm. of Mercury.

Density $= \frac{W}{V} = 0·03458.$

No.	t_1.	t_2.	λ.	w.	Sp. heat.
1	14·93	100·24	536·3	0·1536	0·17615
2	16·52	100·17	536·4	0·1507	0·17629
3	14·94	100·15	536·4	0·1547	0·17766
4	16·28	100·16	536·4	0·1513	0·17658
5	15·18	100·22	536·4	0·1550	0·17835
				Mean......	0·17699

Table II.

Weight of Air in the Sphere = 4·3084 grammes.
Pressure at 100° C. about 21,800 mm. of Mercury.

Density $= \frac{W}{V} = 0·027182.$

No.	t_1.	t_2.	λ.	w.	Sp. heat.
1	16·10	100·22	536·4	0·1194	0·17672
2	15·20	100·33	536·3	0·1222	0·17868
3	16·88	100·33	536·3	0·1182	0·17631
4	15·20	100·15	536·4	0·1199	0·17572
5	16·69	100·12	536·4	0·1188	0·17728
				Mean......	0·17694

Table III.

Weight of Air in Sphere = 3·1357 grammes.
Pressure at 100° C. about 15,890 mm. of Mercury.

Density $= \frac{W}{V} = 0·019784.$

No.	t_1.	t_2.	λ.	w.	Sp. heat.
1	15·88	100·07	536·5	1·0870	0·17680
2	16·69	100·06	536·5	0·0853	0·17506
3	16·43	100·04	536·5	0·0876	0·17926
				Mean......	0·17704

These, it is seen, afford a result above that theoretically assigned to air at constant volume (0·1684). They differ too somewhat from some experiments made by the first-described method, are somewhat lower than their mean, but the consistency displayed throughout in the thirteen experiments given, especially in Tables I and II, leads me to give these numbers as probably a close approximation to the true value. One point is at any rate brought out clearly, that is, that the surmise that the specific heat of a gas at constant volume was a quantity independent of pressure—a surmise based partly on the constancy of the specific heat at constant pressure—would appear to be correct. The values in the three tables, calculated simply on the weights of gas dealt with in each set of experiments, show results quite independent of the great variations of pressure and density obtaining, the weight of condensation simply falling off with the decrease in the weight of gas, till in the third table w is beginning to feel the errors incidental to the considerable mass of the spheres and to give more variable results. I have prepared very thin light spheres with a view to continue the experiments to lower pressures with less danger of error.

The cause of the excess in the value obtained above the theoretical is not apparent, especially in view of the independence of pressure displayed. The experiments embodied in the three tables were made indeed upon the one sample of air—some being liberated after the first five experiments, and so on—but this had been dried through three calcium chloride tubes and two large U-tubes of phosphorus pentoxide before passing into the pump. Between the pump and the sphere it passed through a brass tube stuffed with asbestos which had been previously heated to redness. The object of this is to guard against oil being carried from the pump into the sphere.

My first determination of the specific heat of air at constant volume was effected on the 13th of April of this present year. It was made by the method described in the beginning of this note, at a pressure somewhat higher than that at which the experiments of Table I were effected. This experiment gave as a result the specific heat of air to be 0·17565.

Note. October 18.

Subsequent more extended experiments have shown me that this condition was not absolutely fulfilled. A small reduction of the values recorded for the specific heat of air is necessary on this account, but insufficient to affect any remarks made in this note. Successive experiments on the empty spheres, I may observe, are sufficiently consistent one with another to warrant the assumption that the values recorded by me are not probably affected to the extent of one per cent. by errors on the calorific capacities of the spheres.

II. "Report of Researches on Silicon Compounds and their Derivatives. Part I." By J. EMERSON REYNOLDS, M.D., F.R.S., Professor of Chemistry, University of Dublin. Received September 27, 1888.

The present investigation was undertaken some years ago with a view to examine the action of the silicon haloids—but more especially of silicon tetrabromide—on various compounds containing nitrogen, as our knowledge of the relations of silicon and nitrogen is extremely limited.

It was ascertained at an early stage of the inquiry that the bromide of silicon is much superior to the chloride as a reagent with nitrogenised compounds, but since the bromide had apparently not been obtained in any quantity even by its discoverer, Serullas, considerable time had to be devoted to working out a method for the production of a sufficiently large supply of this material.

In the purification of the crude tetrabromide a new *chlorobromide* of silicon* was discovered, which boils at 141° C. This proved to be the compound $SiClBr_3$, which was required to complete the series of possible chlorobromides of silicon.

The first group of nitrogen compounds subjected to the action of silicon tetrabromide included the primary thiocarbamide or sulphur urea, obtained by the author in 1869, and the allyl-, phenyl-, and diphenyl-thiocarbamides.

All these are shown to unite with silicon tetrabromide and afford the highly condensed compounds—

$$(H_4N_2CS)_8SiBr_4,$$
$$(C_3H_5,H_3N_2CS)_8SiBr_4,$$
$$(C_6H_5,H_3N_2CS)_8SiBr_4,$$
$$((C_6H_5)_2H_2N_2CS)_8SiBr_4.$$

These are more or less vitreous solids, with the exception of the allylic compound, which is a transparent and singularly viscous liquid. All are dissolved and decomposed by water and by alcohol.

The action of alcohol on the compound $(H_4N_2CS)_8SiBr_4$, was studied in detail, and it is shown that not only do ethyl bromide, thiocyanate, and diethylic silicate result, but that the representatives of two new classes of thiocarbamide derivatives are formed.

* The chlorine required for the production of this compound was derived from the crude bromine (which always contains chloride of bromine) used in preparing the tetrabromide.

The first of these is a beautiful *tetrathiocarbamide* compound, whose formula proved to be

$$(H_5N_2CS)_4NBr,$$

which may obviously be written

$$(H_4N_2CS)_4H_4NBr.$$

This body separates from alcohol in fine masses of crystals resembling sea anemones in appearance, which melt at 173—174°, and begin to decompose at 178—180°. The synthesis of this substance was effected by heating ammonium bromide with thiocarbamide.

Several homologues of the above *tetrathiocarbamidammonium bromide* were produced by synthetic methods; some of these contain chlorine or iodine instead of bromine.. The following are examples of the compounds formed in the course of this part of the investigation:—

$$(H_4N_2CS)_4H_4NBr,$$
$$(H_4N_2CS)_4H_4NCl,$$
$$(H_4N_2CS)_4H_4NI,$$
$$(H_4N_2CS)_4(CH_3)H_3NBr,$$
$$(H_4N_2CS)_4(C_2H_5)_2H_2NBr,$$
$$(H_4N_2CS)_4(C_2H_5)_3HNCl.$$

By the action of silver nitrate on the tetrathiocarbamidammonium bromide the crystalline *dithiocarbamide* compound with silver bromide was obtained—

$$(H_4N_2CS)_2AgBr.$$

This was subsequently produced by the direct union of thiocarbamide with the pure silver haloid. The compound—

$$(H_4N_2CS)_2AgCl$$

was also obtained in fine crystals, as were other similar substances.

A *trithiocarbamide* compound is also formed during the action of ethyl alcohol on $(H_4N_2CS)_8SiBr_4$, but it is much more soluble than that which first separates. It is also crystalline, and its analysis and reactions lead to the formula—

$$(H_5N_2CS)_3Br,C_2H_5Br.$$

Hitherto only mono- and di-thiocarbamide derivatives have been known; but the results above stated in outline prove that tri- and tetra-thiocarbamide compounds are formed in presence of silicon tetrabromide and certain other agents, which latter form addition products with the condensed amide.

So far, cases were only dealt with in which silicon tetrabromide combined with nitrogenised groups without loss of its halogen. The next stage of the inquiry involved the investigation of certain interactions in which the tetrabromide loses *all* its halogen. One of the chief results obtained in that direction forms the subject of a separate communication which accompanies this Report.

III. "Preliminary Note on a Silico-organic Compound of a New Type." By J. Emerson Reynolds, M.D., F.R.S., Professor of Chemistry, University of Dublin. Received September 27, 1888.

The subject of this note is a fine crystalline substance, and is the first well-defined compound yet known in which we have reason to believe that silicon is in direct and exclusive union with the nitrogen of amidic groups. Its analysis and mode of formation lead to the conclusion that it is *silicotetraphenylamide*,

$$Si\begin{cases} NHC_6H_5 \\ NHC_6H_5 \\ NHC_6H_5 \\ NHC_6H_5 \end{cases}.$$

This body is produced when silicon tetrabromide (or the tetrachloride) is added to excess of aniline, diluted with three or four volumes of benzene. Aniline hydrobromate (or hydrochlorate) is a secondary product of interaction and separates, being insoluble in benzene. If aniline be in excess throughout the operation, the whole of the halogen precipitates as aniline salt, and there remains in solution impure silicotetraphenylamide. If aniline be not in excess, a bromo-compound is obtained analogous to Harden's rather ill-defined chlorinated product.

Distillation from a water-bath readily removes benzene from the solution, and a liquid remains which solidifies after some time to a yellowish mass. The latter dissolves in warm carbon disulphide leaving a residue containing some thiocarbanilide, and cautious evaporation of the solution leads to the separation of magnificent crystals of the silicon compound. These form chiefly at the surface of the liquid, as they are specifically lighter than the solution.

When twice recrystallised from carbon disulphide, the substance is obtained in a state of purity.*

* A large quantity was prepared in June last, and about 50 grams of the pure compound were exhibited on September 11th, in Section B, during the meeting of the British Association at Bath.

The crystals of silicotetraphenylamide are perfectly colourless short prisms of considerable size. They melt at 136—137° to a transparent liquid, which can be heated to 210° without decomposition. On cooling this liquid solidifies to a transparent glass which, like the original crystals, can be easily decomposed by water.

If silicotetraphenylamide be heated under diminished pressure (about 80 mm.), it affords a distillate of aniline, and leaves a residue which seems to be the silicon analogue of *carbodiphenylimide;* but the latter has not yet been completely analysed.

The detailed investigation of the new substance and its derivatives is in active progress, and promises to throw light on the hitherto obscure relations of silicon and nitrogen.

I have reason to believe that the homologues of aniline, and certain other analogous nitrogen compounds, act like excess of aniline on the silicon haloids, and produce substances similar to the subject of this note. These reactions are also being investigated in my laboratory.

IV. "On the Magnetisation of Iron and other Magnetic Metals in very strong Fields." By J. A. EWING, B.Sc., F.R.S., Professor of Engineering in University College, Dundee, and WILLIAM LOW. Received October 29, 1888.

(Abstract.)

Early in 1887 the authors communicated to the Royal Society the results of experiments made by subjecting iron to strong magnetic force by placing the sample, in the form of a bobbin with a short narrow neck and conical ends, between the pole-pieces of an electromagnet. The experiments have been continued and extended by using much stronger magnetic forces and by testing samples of nickel, cobalt, and various steels, as well as wrought iron and cast iron. The large magnet of the Edinburgh University Laboratory, kindly lent by Professor Tait, was used throughout the experiments, and allowed the authors to effect a high concentration of the magnetic force by using bobbins the necks of which had a cross-sectional area of (in some cases) only $\frac{1}{1100}$ of the cross-sectional area of the magnet cores. By this means the induction $\mathfrak{B}$ was raised to the following extreme values:—

In wrought iron	45,350	c.g.s.
„ cast iron	31,760	„
„ Bessemer steel.......	39,880	„
„ Vickers' tool steel	35,820	„
„ Hadfield's manganese steel....	14,790	„
„ nickel......................	21,070	„
„ cobalt......................	30,210	„

The induction was measured by means of a coil consisting of a single layer of very fine wire wound upon the central neck of the bobbin. Outside of this coil, at a definite distance from it, a second coil was wound, and the magnetic force was determined in the annular space between the two. In a paper communicated to the Manchester meeting of the British Association, the authors showed that if the force so measured could be proved to have the same value as the magnetic force within the metal neck itself, it would follow that the intensity of magnetism $\mathfrak{I}$ had begun to diminish under the action of excessively strong fields, in the manner which Maxwell's extension of the Weber-Ampère theory of molecular magnets anticipates. In the present paper the authors discuss at some length the question of how far the magnetic force within the metal is fairly measurable by the magnetic force in the ring of surrounding air, and they show that with the form of cones originally used the force within the metal must have been less than the force outside, by an amount probably sufficient to explain the apparent decrease of $\mathfrak{I}$. The form of cone suited to give a uniform field of force with sensibly the same value in the metal neck and round it is investigated; and experiments are described in which the condition necessary for a uniform field was satisfied. The results of these experiments are conclusive in showing that no considerable change takes place in the value of $\mathfrak{I}$ (in wrought iron) when the magnetic force is varied from about 2000 to 20000 c.g.s. units. Throughout this range of force, the intensity of magnetism has a sensibly constant value of about 1700 c.g.s. units, which is to be accepted as the saturation value for wrought iron. The term saturation may be properly applied in speaking of the intensity of magnetism, but there appears to be no limit to the degree to which the magnetic induction may be raised.

To produce the greatest concentration of force upon the central neck, the converging pole faces should have the form of cones, with a common vertex in the middle of the neck, and with a semi-vertical angle of 54° 44′. This form, however, does not give a uniform field in the neighbourhood of the vertex. To secure that, the condition is that d^2F/dx^2, d^2F/dy^2, and d^2F/dz^2 shall vanish, F being the magnetic force at the vertex, which is due mainly to the free magnetism distributed over the pole faces. The condition for a uniform field is satisfied when the cones have a semi-vertical angle of 39° 14′. When this form is given to the cones, the magnetic force in the air immediately surrounding the central neck may be taken as sensibly equal to the force within the neck, and it therefore becomes practicable to measure the relation of the induction to the force producing it, that is to say, the magnetic permeability.

The greatest attainable concentration may be calculated by assuming the pole faces to be saturated, when the cones are such as to

have maximum concentrative power (semi-vertical angle = 54° 44′). Under these circumstances the magnetic force at the vertex due to the free magnetism on the conical faces is—

$$18{,}930 \log_{10} \frac{b}{a},$$

where b is the diameter of the poles at the base of the cones, and a the diameter of the central neck.

The following are probable values of the intensity of magnetism when saturation is reached in the particular metals examined:—

	Saturation value of $\mathfrak{I}$.
Wrought iron	1700
Cast iron	1240
Nickel (with 0·75 per cent. of iron)	515
Nickel (with 0·56 per cent. of iron)	400
Cobalt (with 1·66 per cent. of iron)	1300

Experiments were also made with specimens of Vickers' tool steel, and other crucible steels, Whitworth's fluid-compressed steel, Bessemer steel, Siemens steel, and Hadfield's manganese steel. This last material, which is noted for its extraordinary impermeability to magnetic induction, was found to have a constant permeability of about 1·4 throughout the range of forces applied to it, namely, from 2000 to nearly 10,000 c.g.s.

The results are exhibited graphically by curves drawn in Rowland's manner to show the relation of the permeability to the magnetic induction. In the highest field examined, the permeability of wrought iron had fallen to about 2.

V. "The Waves on a rotating Liquid Spheroid of finite Ellipticity." By G. H. Bryan, B.A. Communicated by Professor G. H. Darwin. Received November 6, 1888.

(Abstract.)

The hydrodynamical problem of finding the waves or oscillations on a gravitating mass of liquid which when undisturbed is rotating as if rigid with finite angular velocity in the form of an ellipsoid or spheroid, was first successfully attacked by M. Poincaré in 1885.

In his important memoir "Sur l'Équilibre d'une Masse fluide animée d'un Mouvement de Rotation,"* Poincaré has (§ 13) obtained the differential equations for the oscillations of rotating liquid, and

* 'Acta Mathematica,' vol. 7.

shown that by a transformation of projection, the determination of the oscillations of any particular period is reducible to finding a suitable solution of Laplace's equation.

He then applies Lamé's functions to the case of the ellipsoid, showing that the differential equations are satisfied by a series of Lamé's functions referred to a certain auxiliary ellipsoid; the boundary conditions, however, involving ellipsoidal harmonics referred to both the auxiliary and actual fluid ellipsoids. At the same time, Poincaré's analysis does not appear to admit of any definite conclusions being formed as to the nature and frequencies of the various periodic free waves.

The present paper contains an application of Poincaré's methods to the simpler case when the fluid ellipsoid is one of revolution (Maclaurin's spheroid). The solution is effected by the use of the ordinary tesseral or zonal harmonics applicable to the fluid spheroid and the auxiliary spheroid required in solving the differential equation. The problem is thus freed from the difficulties attending the use of Lamé's functions, and is further simplified by the fact that each independent solution contains harmonics of only one particular degree and rank.

By substituting in the conditions to be satisfied at the surface of the spheroid, we arrive at a single boundary equation. If we are treating the forced tides due to a known periodic disturbing force, this equation determines their amplitude, and hence, the elevation of the tide above the mean surface of the spheroid at any point at any time. If there be no disturbing force it determines the frequencies of the various free waves determined by harmonics of given order and rank. Denoting by κ the ratio of the frequency of the free waves to twice the frequency of rotation of the liquid about its axis, the values of κ are the roots of a rational algebraic equation, and depend only on the eccentricity of the spheroid as well as the degree and rank of the harmonic, while the number of different free waves depends on the degree of the equation in κ. At any instant the height of the disturbance at any point of the surface is proportional to the corresponding surface harmonic on the spheroid multiplied by the central perpendicular on the tangent plane, and is of the same form for all waves determined by harmonics of any given degree and rank, whatever be their frequency, but the motions of the fluid particles in the interior will differ in nature in every case.

Taking first the case of zonal harmonics of the nth degree, we find that according as n is even or odd there will be $\frac{1}{2}n$ or $\frac{1}{2}(n+1)$, different periodic motions of the liquid. These are essentially oscillatory in character, and symmetrical about the axis of the spheroid. In all but one of these the value of κ is essentially less than unity, that is, the period is greater than the time of a semi-revolution of the liquid.

Taking next the tesseral harmonics of degree n and rank s, we find that they determine $n-s+2$ periodic small motions. These are essentially tidal waves rotating with various angular velocities about the axis of the spheroid, the angular velocities of those rotating in opposite directions being in general different. All but two of the values of κ are numerically less than unity, the periods of the corresponding tides at a point fixed relatively to the liquid being greater than the time of a semi-revolution of the mass.

The mean angular velocity of these $n-s+2$ waves is less than that of rotation of the mass by $2/\{s(n-s+2)\}$ of the latter.

In the two waves determined by any sectorial harmonic, the relative motion of the liquid particles is irrotational. The harmonics of degree 2 and rank 1 give rise to a kind of precession, of which there are two.

I have calculated the relative frequencies of several of the principal waves on a spheroid whose eccentricity is $1/\sqrt{2}$.

The question of stability is next dealt with, it being shown that in the present problem, in which the liquid forming the spheroid is supposed perfect, the criteria are entirely different from the conditions of secular stability obtained by Poincaré for the case when the liquid possesses any amount of viscosity, which latter depend on the energy being a minimum. In fact for a disturbance initially determined by any harmonic (provided that it is symmetrical with respect to the equatorial plane, since for unsymmetrical displacements the spheroid cannot be unstable), the limits of eccentricity consistent with stability are wider for a perfect liquid spheroid than for one possessing any viscosity. If we assume that the disturbed surface initially becomes ellipsoidal, the conditions of stability found by the methods of this paper agree with those of Riemann.

The case when the ellipticity, and therefore the angular velocity are very small is next discussed, it being shown that all but two of the waves, or all but one of the oscillations for any particular harmonic become unimportant, their periods increasing indefinitely.

In the case of those whose periods remain finite for a non-rotating spherical mass, the effect of a small angular velocity ω of the liquid is to cause them to turn round the axis with a velocity less than that of the liquid by ω/n.

Finally the methods of treating forced tides are further discussed.

The general cases of a "semi-diurnal" forced tide or of permanent deformations due to constant disturbing forces are mentioned in connexion with some peculiarities they present, and the paper concludes with examples of the determination of the forced tides due to the presence of an attracting mass, first when the latter moves in any orbit about the spheroid, secondly when it rotates uniformly about the spheroid in its equatorial plane.

The effects of such a body in destroying the equilibrium of the spheroid when the forced tide coincides with one of the free tides form the conclusion of this paper.

Presents, November 22, 1888.

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November 30, 1888.

ANNIVERSARY MEETING.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Report of the Auditors of the Treasurer's Accounts on the part of the Society was presented, by which it appears that the total receipts during the past year, including balances carried from the preceding year, amounted to £25,125 18*s*. 6½*d*. on the General Account, and £17,884 0*s*. 7*d*. on account of Trust Funds, and that the total expenditure in the same period, including purchase of stock, amounted to £26,079 0*s*. 0½*d*. on the General Account, and £15,771 14*s*. 6*d*. on account of Trust Funds, leaving on the General Account an overdrawn balance of £953 1*s*. 6*d*., less £22 2*s*. 11*d*. petty cash in hand, and on account of Trust Funds a balance at the Bankers' of £2,112 6*s*. 1*d*.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists:—

Fellows deceased since the last Anniversary (Nov. 30, 1887).

On the Home List.

Burrows, Sir George, Bart., M.D.
Campbell-Johnston, Alexander Robert.
Curling, Thomas Blizard, F.R.C.S.
Farre, Arthur, M.D.
François de Chaumont, Francis Stephen Bennet, M.D.
Frere, George Edward.
Godwin, George, F.S.A.
Gosse, Philip Henry.
Greenhow, Edward Headlam, M.D.
Griess, John Peter, F.C.S.
Hoskins, Samuel Elliott, M.D.
Key, Sir Astley Cooper, Admiral, G.C.B.
Maine, Sir Henry Sumner, K.C.S.I.
Morgan, Octavius S., M.A.
Spratt, Thomas Abel Brimage, Vice-Admiral, C.B.
Stewart, Balfour, M.A.

On the Foreign List.

Clausius, Rudolph Julius Emmanuel.
De Bary, Anton.
Gray, Asa.

Fellows elected since the last Anniversary.

Andrews, Thomas, F.R.S.E.
Balfour, Right Hon. Arthur James.
Bottomley, James Thomson, M.A.
Boys, Charles Vernon.
Church, Arthur Herbert, M.A.
Clarke, Alexander Ross, Colonel, R.E.
Greenhill, Prof. Alfred George, M.A.
Jervois, Sir William F. D., Lieut.-General R.E.
Lapworth, Professor Charles, LL.D.
Macdonald, Right Hon. John Hay Athole.
Parker, Professor T. Jeffery.
Poynting, Professor John Henry, M.A.
Ramsay, Prof. William, Ph.D.
Sudeley, Charles Douglas Richard Hanbury-Tracy, Lord.
Teale, Thomas Pridgin, F.R.C.S.
Topley, William, F.G.S.
Trimen, Henry, M.B.
Ward, Professor Henry Marshall, M.A.
White, William Henry, M.I.C.E.

On the Foreign List.

Becquerel, Edmond.
Kopp, Hermann.
Pflüger, Eduard F. W.
Sachs, Julius.

The President then addressed the Society as follows:—

In the month which intervened between our last anniversary and the end of the year, the Society lost four of its Fellows. In addressing the Fellows last year, I referred to the loss which science had sustained through the death of the illustrious Kirchhoff, and before three weeks were out, one followed him to the grave whose researches on the connexion between the emission and absorption of radiant heat and light were closely akin to those of Kirchhoff. I refer to Balfour Stewart, who, shortly after landing in Ireland, whither he had gone to spend the Christmas with his family, was suddenly carried off after only a few hours' illness, shortly after he had entered on his sixtieth year. His name is widely known on account of his scientific work in heat, magnetism, and solar physics. He has been a member of the Council, and the Rumford Medal of the Society was awarded to him for the particular research to which I alluded at the outset. The other three of our ordinary Fellows who died before the month was out were all far advanced in years. Two of them were eminent in the medical world, Sir George Burrows and Dr. Arthur Farre, both of whom served on our Council. Early in the year we lost one of our

Fellows, who, while not a man of science, was eminent in literature and jurisprudence. While our ranks are mainly recruited from men of science, we gladly welcome among us men who, like Sir Henry Sumner Maine, have proved their ability and earned their distinction in other branches of knowledge, whose connexion with us we look on as honourable to the Society, while, as the very fact of their joining us shows, they regard the Fellowship as honourable to themselves. Admiral Sir Cooper Key, who was highly distinguished as a naval officer, and was at one time Director of the Royal Naval College at Greenwich, was another who served on the Council. Philip Henry Gosse, who died at an advanced age, is well known for his charming popular works on natural history. These are some of the Fellows on the home list who died since the last anniversary; but, besides these, we have lost no less than three of our foreign members. Professor Anton de Bary, so well known for his researches on the cryptogams, and the eminent naturalist, Professor Asa Gray, who not very long ago was over in this country, both died in January. Comparatively recently we have lost Professor Clausius, so eminent as a physicist, especially in the department of thermodynamics.

The year of the Society, which terminates to-day, has shown no flagging in scientific activity. Since the last anniversary, thirty-three memoirs have been published in the 'Philosophical Transactions,' containing a total of 1,010 pages and 91 plates. Of the 'Proceedings,' nineteen numbers have been issued, containing 1,008 pages and 17 plates. In addition to this, a Monograph of the Horny Sponges, by Dr. R. von Lendenfeld, which was accepted for publication by the Council, and which when completed will extend to about 1,000 pages, is now nearly through the press.

A large amount of work connected with the Library has been done since the last anniversary. A special effort has been made to complete imperfect series of scientific periodicals, and by means of exchange, or by the generosity of our corresponding Societies, some hundreds of deficient numbers have been obtained. The Lists of Institutions entitled to receive gratis the 'Philosophical Transactions' and 'Proceedings' have also been carefully revised by the Library Committee.

In December last, Mr. Arthur Soper was engaged as a special Assistant to continue the formation of the Shelf-Catalogue, and the revision of the Catalogue of MSS., and for other work. The Shelf-Catalogue of the Upper Library is now completed—a work involving the re-arrangement or removal to the lower storeys of several thousands of volumes. Considerable progress has been made in collating and cataloguing the Archives and other manuscripts belonging to the Society, and an instalment of slips have been written towards a Catalogue of the Miscellaneous Literature in the Library.

In the course of this work many duplicate scientific books, and literary works of little value to the Society, have been thrown out, and these have been presented, by order of the Council, to the libraries of the Universities and some of the chief Scientific Societies.

The cataloguing of the titles of scientific papers for the decade 1874 to 1883 is now complete, and the work is ready for the press. The amount of matter is estimated to require, if printed, three quarto volumes of the usual size. The extraction of the titles, the preparation of the work for the press, and the correction of the proofs of this work, which is really of international importance, have all along been done at the sole charge of the Royal Society; but the printing of the volumes which have already been published has been done at the Stationery Office, by authority of the Lords of the Treasury, and the proceeds of the sale have been paid in to the Treasury. The Council have applied to the Lords Commissioners of Her Majesty's Treasury to sanction the printing of the last decade in a similar manner, and it is hoped that the application may be favourably entertained.

In the year 1882 a change was made in the amount and mode of administration of the Grant, which for a considerable time before had been voted annually by Parliament for scientific research. Since that year the annual grant has been one of £4,000, which has been administered by the former Government Grant Committee, with the addition of certain *ex-officio* members, mostly the Presidents of certain scientific Societies. Meetings of this large Committee, consisting usually of about 50 members, have been held twice a year, the various applications for aid from the Grant to enable the applicant to carry out investigations explained by him having been previously discussed in meetings of three, or latterly two, Sub-Committees, into which the whole Committee was divided, and then been submitted to the General Committee for confirmation or modification.

In the discussion of these Grants, the Government received the benefit of the gratuitous services of a large number of men of the highest distinction in science. In the large Sub-Committees, however, it necessarily happened that of the members present only a fraction would be likely to be conversant with the particular branch of science to which any particular application belonged; and the Council thought that the time of the members might be economised, and at the same time a more efficient discussion of the Grants secured, by arranging the applications under a number of sub-divisions, and assigning the discussion of these to a corresponding number of Boards formed out of the General Committee. It was thought that a good deal of the discussion of the applications in the several branches might be carried on by correspondence among the members of the respective Boards, so that one or two meetings of each Board might suffice. If some trouble were thus saved to the members of the

Committee in regard to personal attendance at long meetings, there would probably be more expenditure of time in the way of correspondence, and it was thought that one meeting of the General Committee in the year would in most cases suffice. To meet pressing cases in the interval, it was suggested that a limited sum might be placed by the General Committee at the disposal of the Council of the Royal Society. There are further provisions for forming a reserve fund of not more than £2,000 to meet special objects involving unusual expenditure, and for holding in reserve out of the money available for any one year enough to meet annual grants of limited amount made for a period not exceeding three years, the future grants being contingent on the receipt by the Committee of satisfactory evidence of progress in the inquiry. The new regulations, of which I have merely given a slight sketch, have been communicated to the Treasury, and will come into operation next year.

The Krakatoa Committee have now completed their work, and the volume which is the outcome of their labours is in the hands of the public. It has been favourably noticed in more than one quarter. The Society is much indebted to those Fellows and other gentlemen who discussed and reported on the different subjects into which the whole inquiry was divided, and to Mr. Symons, who was the first to propose that the materials should be collected, and to whose unwearied labour as Chairman of the Committee, director of the correspondence, and editor of the volume, the successful accomplishment of the undertaking is largely due. A comprehensive and digested account of that extraordinary volcanic explosion, remarkable both for its magnitude and the striking disturbances and other phenomena attending or following it, is now placed within easy reach of the ordinary reader, and will go down to posterity, whereas, had the various accounts remained in their isolated form, they would many of them have perished, and the remainder could not have been brought together without a most laborious search. It must be a great satisfaction to my predecessor in this chair to remember that he urged upon the Council the importance of collecting the facts before the materials should have become dissipated, and while the freshness of men's recollection of the event kept up a lively interest in all that belonged to it.

The Royal Society is in possession of some important standards for the safe keeping of which we are responsible. Parliamentary copies of the standard yard and standard pound have been entrusted to our custody; and we have also a standard measure of length known as Sir George Schuckburgh's scale, with reference to which the length of the seconds' pendulum for Greenwich has been determined by Kater and Sabine. This length, as determined by experiment, has been with reference to the interval from the 0 to the 39 and

40-inch graduations on the scale; but no exact comparison has hitherto been made between the length of this portion of the scale and the national yard, and such a comparison is no easy matter. It happens that Commandant Deforges has been engaged in determining the length of the seconds' pendulum at Greenwich with reference to the French standard metre; and just before his return to Paris he came to our meeting, and offered to take charge of the scale, bring it with him to Paris, and there determine the length of the part of the scale used by Kater and Sabine with reference to the metre, for doing which he has all the requisite appliances; and as we know the ratio of the metre to the yard, the length of the seconds' pendulum as determined by Kater and Sabine would thus be known accurately with reference to the standard yard. It seemed to me that so important a scale should hardly be sent away, even though in the care of so experienced a physicist, without the authority of the Council, and without an outer case being made for its box, which there was not time to get ready. The authority of the Council has since been obtained, and it fortunately happens that one of the assistants at the Greenwich Observatory is going to Paris, who will take charge of the scale. Thus by the kind proposal of Commandant Deforges, we may shortly hope to have an authentic comparison of the length of the seconds' pendulum as measured by Kater and Sabine with the standard English yard.

At the time of the anniversary last year, some of the reports of the observers who went to Grenada to observe the Total Solar Eclipse of August, 1886, had been sent in, and I mentioned that it seemed desirable, for convenience of reference at a future time, that the different reports should come out together, instead of being published in a scattered form, provided at least that the waiting for the later reports should not cause too much delay. I regret to say that the completion of the reports has been delayed in part by the illness of one of the observers, but I have every hope that they will all be in by Christmas, and I do not anticipate that any long time will elapse before they will be in some form in the hands of the public.

The time is well within our recollection when the occurrence of the solar prominences seen in total eclipses first attracted the attention of astronomers, and when, for observations bearing on their nature, we had to wait for the rare and brief glimpses which, clouds permitting, were afforded by total eclipses. Now, however, thanks to the method of observation devised independently by Lockyer and Janssen, they may be studied at any time. It would obviously be a great advantage if a similar study could be made of the corona; for though we cannot expect to obtain a picture of it equal to that which may be got during a total eclipse, yet if a fairly good picture could be obtained from time to time, we might thereby be enabled to learn

more about the history of its changes than could be got by observations extending over a lifetime if restricted to total eclipses. Some observations were made during the partial phases of the last total eclipse with the view of throwing light on the prospect of success. Notwithstanding the unpromising nature of the results obtained, I have reason for hoping that the desired object may yet be accomplished.

In addressing you last year, that year which will be memorable as the Jubilee of the reign of our beloved Sovereign, I alluded briefly to the progress which science had made in the last half century, and ventured to indicate one or two directions in which it seemed to me possible that a very great addition to our physical knowledge might some day be reached. I will not to-day venture to look so far ahead; but the mention of a total eclipse leads me to refer to some theories now before the scientific world which are likely to undergo full discussion and further examination in the near future, with the probable result of a pretty general agreement as to their acceptance or rejection.

It is now many years since Dr. Huggins discovered the peculiar character of the spectra of the nebulæ, spectra which he found to consist mainly of bright lines, indicating that what we see is an incandescent gas. The natural supposition to make at the time was that those distant masses of matter consisted of incandescent gas, of which the luminosity was in some way kept up, probably as a result of condensation. But the researches of Mr. Lockyer, as described by him in the Bakerian lecture which he delivered last spring, and in part in a previous paper communicated shortly before the last anniversary, have led him to take a different view of the constitution of nebulæ. According to the theory advanced by him, the mass of a nebula consists mainly of meteorites, which are constantly coming into collision here and there; and the glowing gas the existence of which the spectroscope reveals, is merely a portion of the matter, volatilised by the heat of collision. According to the former view therefore, the nebula consists of glowing gas, not yet condensed into a solid or liquid form, possibly in a condition even more elementary than that of the so-called elements that we know on earth; according to the latter it consists mainly of discrete portions of solid matter, and the glowing gas does not consist of the same matter permanently glowing, but is continually supplied afresh by fresh collisions.

A similar theory is applied to explain the self-luminosity of the nucleus, and sometimes the very root of the tail, of comets. A comet is regarded as a swarm of meteorites, moving in orbits not greatly differing from one another; and as the swarm approaches the sun collisions become more frequent, and individually more potent, from an increase in the velocities differential as well as absolute; and

a portion of matter is volatilised and rendered incandescent. As to the tail, the theory long ago suggested by Sir John Herschel has always seemed to me by far the most probable of those that have been advanced, namely, that it is due to the propulsion of excessively attenuated matter, owing to a repulsive force, probably of electrical origin, emanating from the sun. This view seems to be adopted both by Mr. Lockyer and Dr. Huggins; and the latter gentleman in an earlier Bakerian lecture has suggested a new theory of the corona—the corona as distinguished from the prominences—namely, that it is projected from the sun by molar forces due to the tremendous state of turmoil, in which we have very strong reason for believing that the matter composing the sun exists, but of matter actually propelled from the sun by a repulsive force in the manner of the tails of comets.

Daring as some of these speculations may appear to be, there seems a great deal to recommend them, and the whole subject is one of extreme interest at the present day.

But I must not take up your time longer by dwelling on so special a subject; I proceed to matters more particularly connected with the occasion on which we are assembled.

The Council have awarded the Copley Medal of the year to my predecessor in this chair, Mr. Huxley, for his investigations on the morphology and histology of vertebrate and invertebrate animals, and for his services to biological science in general during many past years. These subjects lie so entirely out of the range of my own studies that I need hardly say that in attempting to give some idea of the more salient features of his investigations I am dependent upon the kindness of biological friends.

During the fifteen or twenty years which preceded the publication of Darwin's famous work, the 'Origin of Species,' the views and methods of comparative anatomists underwent a most marked change. Without that change biologists would have been far less prepared to accept Mr. Darwin's work, and, what is even more important, would have been unprepared to make use of that work as a light enabling them to carry on the remarkable researches which have so brilliantly characterised the progress of biology during the last quarter of a century. That change was effected chiefly by the labours first of Johannes Müller, and subsequently of Huxley in this country, and of Gegenbaur in Germany. The labours of these men opened out the right road of morphological inquiry. It is not, perhaps, too much to say that Mr. Huxley's treatment of his subject in his 'Morphology of Cephalous Mollusca' was to many young morphologists little short of a revelation, and all his other works of the same period, such as that on the hydrozoa and on tunicates, and latter still his treatment of the vertebrate skull and skeleton, and arthropoda produced in varying degree a like effect.

Closely allied to, or rather forming part of, his morphological labours are his numerous palæontological researches, carried out for the most part while he was palæontologist to the Geological Survey, researches characterised by the same clear morphological insight, researches which have been as profitable to animal morphology as useful to the geologist. The most important are perhaps those on the remarkable reptiles of the Elgin Sandstones and on the Dinosauria; but many others have great value, and his Anniversary Address to the Geological Society, in 1870, made its mark.

Though his career has been in the main that of a morphologist, he has through the common ground of histology given considerable help to physiology. An early paper by him 'On the Cell-Theory,' did much to clear away erroneous notions concerning the relations of structure to the actions of living beings. His article on 'Tegumentary Organs' was a great step onward as regards both morphology and histology, and still remains a classical work; while, by other papers and in various ways, he has contributed to the progress of histology and physiology.

But however important Mr. Huxley's original contributions to the advancement of our scientific knowledge have been, we should form a very inadequate idea of his benefits to the cause of science if we did not bear in mind also his singular ability and effectiveness as an expositor of science to the people, and the powerful influence he has exerted in the improvement of the teaching of biology in its widest sense in this country. Indeed, it is not too much to say that the remarkable improvement which has taken place within the last few years must be ascribed either directly or indirectly to his influence, and has been in many cases due to his initiation.

The Rumford Medal has been awarded to Professor Pietro Tacchini for important and long-continued investigations, which have largely advanced our knowledge of the physics of the sun.

Professor Tacchini occupies a foremost place among those who have paid special attention to the physics of the sun. Since 1870 he has singly observed, first at Palermo, and afterwards at Rome, the solar prominences. The information at our disposal at the present time, both as regards their distribution, their spectra, and the changes which take place in them, and their connexion with other solar phenomena, rests to a large extent upon his individual efforts. His memoirs on this subject are very numerous. He has been engaged in the observation of four total solar eclipses, and from some of the phenomena therein observed has drawn the important conclusion that many of the so-called prominences are really descending currents.

A Royal Medal has been awarded to Sir Ferdinand von Mueller for his long services in Australian exploration, and for his investigations of the flora of the Australian continent.

For more than forty years von Mueller has been working, without intermission, at scientific botany and its practical illustrations. As a botanical traveller and collector, he has, to quote the words of Sir Joseph Hooker, "personally explored more of the Australian continent than any other botanist, except Allan Cunningham." No one has investigated the Australian flora and the geographical distribution of its components with so much perseverance and success, and no one has enriched our herbaria, laboratories, and gardens with materials for study to so great an extent. The eleven volumes of the 'Fragmenta Phytographiæ Australiæ' contain the descriptions of a great series of new plants, and the unrestricted communication of his collections and observations to the late Mr. Bentham rendered possible the preparation of the 'Flora Australiensis,' in seven volumes, the only account of the vegetation of any large continental area which has at present been completed.

He has especially devoted himself to the elucidation of the most difficult, though most characteristic groups of the Australian flora; and as a result of his labours in this direction, his 'Eucalyptographia' may be more particularly mentioned, a work which will always be the standard of nomenclature for the intricate genus *Eucalyptus.* Of a similar character are his descriptions and illustrations of the 'Myoporineous Plants of Australia,' and his 'Iconography of the Genus *Acacia.*' To him is also due the foundation of the Government Herbarium at Melbourne, the first great botanical collection formed in the southern hemisphere, and the future centre of all scientific work on the Australasian flora.

A Royal Medal has been awarded to Professor Osborne Reynolds for his investigations in mathematical and experimental physics, and on the application of scientific theory to engineering.

Professor Reynolds was among the first to refer the repulsion exhibited in that remarkable instrument of Mr. Crookes's, the radiometer, to a change in the molecular impact of the rarefied gas consequent upon the slight change of temperature of the movable body due to the radiation incident upon it; and in an important paper published in the 'Philosophical Transactions' for 1879, he deduced from theoretical considerations the conclusion that similar phenomena might be expected to be observed in bodies surrounded by a gas of comparatively large density, provided their surfaces were very small. He verified this anticipation by producing on silk fibres, surrounded by hydrogen at the atmospheric pressure, impulsions similar to those which in a high vacuum affect the relatively large disks of the radiometer.

In an important paper published in the 'Philosophical Transactions' for 1883, he has given an account of an investigation, both theoretical and experimental, of the circumstances which determine whether the

motion of water shall be direct or sinuous, or, in other words, regular and stable, or else eddying and unstable. The dimensions of the terms in the equations of motion of a fluid when viscosity is taken into account involve, as had been pointed out, the conditions of dynamical similarity in geometrically similar systems in which the motion is regular; but when the motion becomes eddying it seemed no longer to be amenable to mathematical treatment. But Professor Reynolds has shown that the same conditions of similarity hold good, as to the average effect, even when the motion is of the eddying kind; and moreover that if in one system the motion is on the border between steady and eddying, in another system it will also be on the border, provided the system satisfies the above conditions of dynamical as well as geometrical similarity. This is a matter of great practical importance, because the resistance to the flow of water in channels and conduits usually depend mainly on the formation of eddies; and though we cannot determine mathematically the actual resistance, yet the application of the above proposition leads to a formula for the flow, in which there is a most material reduction in the number of constants for the determination of which we are obliged to have recourse to experiment.

There are various other investigations of Professor Reynolds's which time would not allow me to enter into, and I therefore merely mention his investigation of the relation between rolling friction and the distortion produced by the rolling body on the surface on which it rests, that of the effect of the change of temperature with height above the surface of the ground on the audibility of sounds and his explanation of the effect of lubrication as depending on the viscosity of the lubricant.

The Davy Medal has been awarded to Mr. Crookes for his investigations on the behaviour of substances under the influence of the electric discharge in a high vacuum.

Mr. Crookes's remarkable series of researches which conducted him to the invention of the radiometer led him to work with excessively high vacua. In connexion with this he found that an electric discharge in such vacua is capable of exciting effects of phosphorescence apparently quite different in their origin from those produced in the ordinary way by such discharges. The latter are clearly referable to the action of the ethereal undulations which are propagated from the seat of the discharge. But the former involve in some way the effect of the actual transference of the molecules of ponderable matter. These phenomena, in the hands of Mr. Crookes, opened up a new means of discrimination between different bodies, and he has applied them as a test for the discrimination of groups of rare earths, not yet fully investigated. The test went hand in hand with processes of chemical separation. But here a great difficulty

presented itself. So very closely allied in their chemical properties are the members of the groups, that it was only by an excessively tedious and laborious system of fractional precipitation that Mr. Crookes was able to effect a pretty fair separation. Even still, the separate existence of some members of the groups is more or less problematical. It is for these most painstaking researches that the medal has been awarded.

The existence, or apparent existence, of so many earths of such close chemical relationship led Mr. Crookes to speculate on the possibility that after all the molecules of what is deemed a chemical element may not be absolutely alike, as chemists have almost universally believed, but only very approximately so, and that what is deemed the molecular weight of the substance may really be that of the average of its molecules. Should such groups exist, it is conceivable that by processes of very delicate chemical separation they might be split up again into sub-groups, the molecules of which still more nearly match one another; so that according to this view the number of groups into which an element, or what is deemed such, might be split up, not, be it observed, by any dissociation, but merely by a sorting of the molecules which are very nearly alike, may be somewhat indefinite.

Chemists will not probably be disposed to give up the idea of the perfect similarity of the individual molecules of elementary bodies; but it is surely legitimate for one who has worked so assiduously at these difficult separations to suggest, merely as a matter for chemists to think about, a possible view of the nature of elements different from that to which they have been accustomed.

The Statutes relating to the election of Council and Officers were then read, and Sir James Cockle and Professor Rücker having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Professor George Gabriel Stokes, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.—Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.

Other Members of the Council.

Professor Henry Edward Armstrong, Ph.D.; Henry Bowman

Brady, F.G.S.; Charles Baron Clarke, M.A.; William Huggins, D.C.L.; John Whitaker Hulke, F.R.C.S.; Professor John W. Judd, F.G.S.; Edward Emanuel Klein, M.D.; Professor E. Ray Lankester, M.A.; Professor Herbert McLeod, F.I.C.; Sir James Paget, Bart., D.C.L.; William Pole, Mus. Doc.; William Henry Preece, M.I.C.E.; Sir Henry E. Roscoe, D.C.L.; Edward John Routh, D.Sc.; Professor Arthur William Rücker, M.A.; William James Lloyd Wharton, Capt. R.N.

The thanks of the Society were given to the Scrutators.

Balance Sheet, 1888.

Statement of Receipts and Expenditure from November 12th, 1887, to November 12th, 1888.

	£	s.	d.	£	s.	d.
To Balance at bank, 12th November, 1887				232	0	5
„ Balance in hand, Catalogue Account	14	9	7	18	5	11¼
„ „ Petty Cash	3	16	4¼			
„ Annual Contributions, 163 at £4	652	0	0	1,036	0	0
„ „ 128 at £3	384	0	0			
„ Admission Fees				30	0	0
„ Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions				288	0	0
„ Rents:						
Fee Farm, Lewes	18	14	5	82	0	3
Mablethorpe Estate	63	5	10			
„ Ground Rents				598	2	7
„ Dividends (exclusive of Trust Funds)				2,050	19	4
„ do. Jodrell Fund				151	5	4
„ Interest on Mortgage Loan				583	8	9
„ Sale of Transactions and Proceedings				594	6	5
„ Bonus on conversion of Consols				10	6	7
„ do. Reduced 3 per Cents				38	15	0
„ Sale of £4,133 6*s.* 8*d.* Consols (Converted)				4,086	16	2
„ do. £15,499 19*s.* 9*d.* Reduced 3 per Cents (Converted)				15,325	11	9
„ Bankers, Balance overdrawn				953	1	6
				£26,079	0	0¼

	£	s.	d.	£	s.	d.
By Salaries, Wages, and Pension				1,675	3	8
„ Catalogue of Scientific Papers				165	11	4
„ Books for the Library				249	18	9
„ Printing and Advertising Transactions, and Separate Copies to Authors and Publisher	362	4	1	2,593	10	8
„ Ditto Proceedings, Nos 258 to 270	539	10	5			
„ Ditto Miscellaneous	113	8	1			
„ Paper for Transactions and Proceedings	499	15	2			
„ Binding ditto	50	17	1			
„ Engraving and Lithography	1,027	15	10			
„ Soirée and Reception Expenses				109	1	4
„ Coal, Lighting, &c.	141	19	9	861	6	2¼
„ Office Expenses	58	5	0¼			
„ House Expenses	267	18	4			
„ Tea Expenses	18	3	5			
„ Fire Insurance	56	15	0			
„ Taxes	51	8	8			
„ Advertising	21	1	6			
„ Postage, Parcels, and Petty Charges	50	0	0			
„ Miscellaneous Expenses	195	14	6			
„ Law Charges				49	12	8
„ Electric Lighting, Installation				79	0	8
„ Lendenfeld Monograph (making with previous expenditure £296 9*s.* 2*d.*)				248	4	2
„ Purchase of £12,150 London and North Western Railway 4 per Cent. Guaranteed Stock				15,364	5	9
„ Purchase of £3,333 London and South Western Railway 4 per Cent. Preference Stock				4,097	2	10
„ Krakatoa Report, excess of expenditure over receipts to date				516	17	1
„ Eclipse Expedition (making with previous expenditure a total of £207 11*s.* 7*d.* in excess of receipts)				2	2	0
„ Carrington Donation				45	0	0
„ Balance on hand, Catalogue Account	£14	1	7	22	2	11
„ Ditto, Petty Cash	8	1	4			
				£26,079	0	0¼

Trust Funds.

	£	s.	d.	£	s.	d.
To Balance at Bank 12th November, 1887:—						
General Account	986	12	0			
Fee Reduction Fund Account	176	4	6			
Scientific Relief Fund Account	573	14	5	1,736	10	11
„ Scientific Relief Fund, Dividends, Donations, and Sale of Stocks	7,768	5	11			
„ Donation Fund, Dividends, Sale of Stocks, &c., and Transfer from Handley Fund	6,768	3	6			
„ Rumford Fund, Dividends	90	10	6			
„ Bakerian and Copley Medal Fund, Dividends, &c.	74	15	3			
„ Keck Bequest, Dividends	23	6	6			
„ Wintringham Fund, Dividends, &c.	46	15	3			
„ Davy Medal Fund, Dividends	32	2	1			
„ Gassiot Trust, Dividends, &c.	506	3	9			
„ Jodrell Fund, Dividends	151	5	4			
„ Fee Reduction Fund, Dividends	403	6	8			
„ Darwin Memorial Fund, Dividends, and Donation	277	14	11	16,147	9	8
				£17,884	0	7

	£	s.	d.	£	s.	d.
By Scientific Relief Fund, Investments, and Grants	7,989	19	6			
„ Donation Fund, Grants, Investments, &c.	6,345	18	9			
„ Bakerian and Copley Medal Fund, Medal, and Gift	58	12	0			
„ Keck Bequest, Payment to Foreign Secretary	23	6	6			
„ Wintringham Fund, Payment to Foundling Hospital	34	17	4			
„ Croonian Lecture Fund, Payments	53	18	10			
„ Davy Medal Fund, Gold Medals	32	4	6			
„ Gassiot Trust, Payments to Kew Committee	492	10	9			
„ Jodrell Fund, Transfer to Royal Society General Account	151	5	4			
„ Fee Reduction Fund, transfer to Royal Society General Account (1888)	288	0	0			
„ Darwin Memorial Fund, Expenses	301	1	0	15,771	14	6
„ Balance at Bankers:—						
General Account	1,463	14	1			
Fee Reduction Fund Account	236	11	2			
Scientific Relief Fund Account	352	0	10	2,112	6	1
				£17,884	0	7

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £100 per annum.

Ground Rent of House No. 57, Basinghall Street, rent £380 per annum.

" " of 23 houses in Wharton Road, West Kensington, rents £253 per annum.

Fee Farm Rent, near Lewes, Sussex, £19 4*s*. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, about £52 per annum, Croonian Lecture Fund.

Stevenson Bequest. Chancery Dividend. One-fourth annual interest on Government Annuities and Bank Stock (produced £666 18*s*. 1*d*. in 1887-88).

£15,000 Mortgage Loan, 4 per Cent.

£14,174 8*s*. 3*d*., $2\frac{3}{4}$ per Cent. Consolidated Stock, being £10,722 7*s*. 2*d*., namely:—

	£	*s*.	*d*.
Rumford Fund	2,322	19	0
Wintringham Fund	1,200	0	0
Gassiot Trust	350	0	0
Sir J. Copley Fund	1,666	13	4
Jodrell Fund	5,182	14	10

and £3,452 1*s*. 1*d*. in Chancery, arising from sale of the Coleman Street Estate.—General Purposes.

£408 9*s*. 8*d*. New $2\frac{1}{2}$ per Cent. Stock.—Bakerian and Copley Medal Fund.

£1,000 India $3\frac{1}{2}$ per Cent. Stock.—General Purposes.

£600 Midland Railway 4 per Cent. Debenture Stock.—Keck Bequest.

£5,660 Madras Railway Guaranteed 5 per Cent. Stock $\begin{cases}\text{General Purposes, £5,000.}\\ \text{Davy Medal Fund, £660.}\end{cases}$

£10,000 Italian Irrigation Bonds.—The Gassiot Trust.

£6,396 Great Northern Railway 4 per Cent. Debenture Stock $\begin{cases}\text{Scientific Relief Fund, £5,000.}\\ \text{The Trevelyan Bequest, £1,396.}\end{cases}$

£4,000 Metropolitan 3½ per Cent. Stock.—Fee Reduction Fund.

£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.—Fee Reduction Fund.

£18,150 " " " " 4 per Cent. Consolidated Guaranteed Stock.—{ £6,000 Scientific Relief Fund. £12,150 General Purposes.

£5,000 North Eastern Railway 4 per Cent. Preference Stock.—General Purposes.

£5,000 London and North Western Railway Consolidated 4 per Cent. Preference Stock.—General Purposes.

£2,000 South Eastern Railway 4 per Cent. Debenture Stock.—Darwin Memorial Fund.

£4,340 South Eastern Railway 5 per Cent. Debenture Stock.—Scientific Relief Fund.

£3,333 London and South Western Railway 4 per Cent. Preference Stock.—General Purposes.

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.—Donation Fund.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.—Handley Fund.

JOHN EVANS, *Treasurer.*

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct.

G. G. STOKES.
WILLIAM POLE.
GEORGE HENRY RICHARDS.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct.

JOHN BALL.
JAMES COCKLE.
WILLIAM HUGGINS.
JOHN RAE.
G. J. SYMONS.

Trust Funds. 1888.

Scientific Relief Fund.

£6,000 L. & N.W.R. 4 per Cent. Consolidated Guaranteed Stock.
£5,000 Great Northern Railway 4 per Cent. Debenture Stock.
£4,340 South Eastern Railway 5 per Cent. Debenture Stock.

Dr.

	£	s.	d.	£	s.	d.
To Balance { Capital	286	12	9			
To Balance { Income	287	1	8			
				573	14	5
„ Dividends, 1888				623	11	5
„ Annual Subscriptions				6	1	0
„ Curling Bequest				200	0	0
„ Sale of £7,000 New 3 per Cent. Annuities (converted)				6,938	13	6
				£8,342	0	4

Cr.

	£	s.	d.	£	s.	d.
By Grants				522	0	0
„ Purchase of £400 L. & N. W. Ry. 4 per Cent. Consolidated Guaranteed Stock				524	10	0
„ Purchase of £4,340 South Eastern Railway 5 per Cent. Debenture Stock				6,943	9	6
„ Balance in hand, Income	£388	13	1			
„ Less—Capital over-invested	36	12	3	352	0	10
				£8,342	0	4

Donation Fund.

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.
The Trevelyan Bequest. £1,396 Great Northern Railway 4 per Cent. Debenture Stock.

Dr.

	£	s.	d.
To Balance	110	0	5
„ Dividends, 1888	239	6	11
„ Sale of £6,339 0s. 1d. Consols (Converted)	6,267	13	3
„ Bonus on ditto	15	17	0
„ Amount returned by Dr. Hirst	13	17	9
„ Transfer from Handley Fund	181	8	7
	£6,878	3	11

Cr.

	£	s.	d.
By Grants	61	18	6
„ Purchase of £4,023 Great Northern Railway Perpetual 5 per Cent. Guaranteed Stock	6,283	10	3
„ Binding		10	0
„ Balance	532	5	2
	£6,878	3	11

Rumford Fund.

£2,322 19*s*. 2¾ per Cent. Consolidated Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	68	0	3	By Balance	158	10	9
„ Dividends, 1888	84	14	6				
„ Bonus on conversion of Consols	5	16	0				
	£158	10	9		£158	10	9

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift, £1,666 13*s*. 4*d*. 2¾ per Cent Consolidated Stock.
£403 9*s*. 8*d*. New 2½ per Cent. Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	105	2	7	By Gold Medal	4	12	0
„ Dividends, New 2½ per Cent. Stock, 1888	9	16	4	„ Sir J. D. Hooker—Sir J. Copley's Gift	50	0	0
„ Dividend—Sir J. Copley's Fund, 1888	60	15	5	„ Professor J. N. Lockyer—Bakerian Lecture	4	0	0
„ Bonus on conversion of Consols—Sir J. Copley's Fund	4	3	6	„ Balance	121	5	10
	£179	17	10		£179	17	10

The Keck Bequest.

£600 Midland Railway 4 per Cent. Debenture Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Dividends, 1888	23	6	6	By Payment to Foreign Secretary	23	6	6

Wintringham Fund.

£1,200 2¼ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	34	17	4	By Payment to Foundling Hospital, 1888	34	17	4
„ Dividends, 1888	43	15	3	„ Balance	46	15	3
„ Bonus on conversion of Consols	3	0	0				
	£81	12	7		£81	12	7

Croonian Lecture Fund.

One-fifth of the clear rent of an Estate at Lambeth Hill, from the College of Physicians, about £52 per annum.

	£	s.	d.		£	s.	d.
To Balance, November, 1887	48	2	10	By Lecture—Professor H. G. Seeley	48	2	10
„ Ditto November, 1888	5	16	0	„ Assistants, Translating, &c.	5	16	0
	£53	18	10		£53	18	10

Davy Medal Fund.

£660 Madras Railway Guaranteed 5 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	77	3	6	By Gold Medals	32	4	6
„ Dividends, 1888	32	2	1	„ Balance	77	1	1
	£109	5	7		£109	5	7

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

£350 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	38	7	6	By Payments to Kew Committee	492	10	9
„ Dividends, 1888	505	6	3	„ Balance	52	0	6
„ Bonus on conversion of Consols		17	6				
	£544	11	3		£544	11	3

Handley Fund.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.

	£	s.	d.		£	s.	d.
To Dividends, 1888	181	8	7	By Purchase of £4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock	5,994	8	11
„ Bonus on conversion of Reduced 3 per Cent. Annuities	15	2	4	„ Transfer to Donation Fund	181	8	7
„ Sale of £6,047 7*s.* 9*d.* Reduced 3 per Cent. Annuities	5,979	6	7				
	£6,175	17	6		£6,175	17	6

The Jodrell Fund.

£5,182 14*s.* 10*d.* 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Dividends, 1888	151	5	4	By transfer to Royal Society General Account	151	5	4

Fee Reduction Fund.

£4,000 Metropolitan 3½ per Cent. Stock.
£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	176	4	6	By transfer to Royal Society General Account (1888)	288	0	0
„ Dividends, 1888	408	6	8	„ Balance	296	11	2
	£584	11	2		£584	11	2

Darwin Memorial Fund.

£2,000 South Eastern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	504	17	7	By Westminster Abbey Fees	151	1	0
„ Dividends, 1888	77	14	11	„ Medallion—J. E. Boehm, R.A.	150	0	0
„ Donation	200	0	0	„ Balance, Capital £249 1 8; Interest £232 9 10	481	11	6
	£782	12	6		£782	12	6

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Royal	Foreign.	Compounders	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1887 ..	5	48	188	165	112	518
Since Elected ..	.	+ 4	+ 0	+ 3	+ 16	+ 23
Since Deceased ..	..	− 3	− 6	− 8	− 1	− 18
Nov. 30, 1888 ..	5	49	182	160	127	523

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the advancement of Science (continued from Vol. XLIII, p. 205).

1887–88.

	£
J. Murray, for an Examination of the Western Lochs of Scotland	400
W. G. Forster, for cost of a Seismograph to be used in a Research on the laws which regulate Earthquake Motion	75
F. R. Japp, for an Investigation of the Reactions of Ketones, Diketones, and allied compounds..........	75
Hon. R. Abercromby, for the Systematic Observation of British Thunderstorms	25
W. R. Dunstan, for the Investigation of the Reduction of the Nitro-paraffins and Alkyl Nitrites as effected by Ferrous Hydroxide	30
J. Croll, for books and payment of a Secretary to aid in completing a work on the fundamental principles which underlie the Doctrine of Evolution in its widest sense..........	25
H. R. Mill, to discuss the Observations of Temperature made by the Staff of the Scottish Marine Station in the Clyde-sea area	100
Dr. T. Lauder Brunton, for Investigations on the connexion between Chemical Constitution and Physiological Action	100
Carried forward..........	£830

Brought forward................	£830
Dr. F. Warner, to complete apparatus for enumerating combinations of Movements in the Human Body................	60
Dr. L. C. Wooldridge, for further Research on the Physiology and Pathology of the Blood	40
J. Beard, for further Research in Elasmobranch and Ganoid Development	80
A Committee of the Royal Society, for continuing the boring in the Delta of the Nile................................	500
W. H. Pendlebury, for an Investigation of a case of gradual Chemical Change, viz., that between Hydrogen Chloride and Potassium Chlorate.......................................	50
G. Massee, to prepare a Monograph of the Fungi belonging to the order Thelephorei	100
F. J. Smith, for a Research on the Acceleration Period of the Explosion, in tubes, of Gaseous Mixtures, and the point or points at which the explosion is propagated at its maximum velocity..	50
R. Meldola, for a Research on Diazo-compounds	20
J. N. Lockyer, for Aid in providing a large Reflecting Telescope, and to pay an Assistant in a Research on the exact sequence of temperature phenomena in meteoric swarms......	300
C. Piazzi Smyth, for Researches in Spectroscopic Measurement of Ultra Definition and Extreme Separation	100
C. V. Boys, to Investigate, if possible, the Heat sent to the Earth from the Stars, Planets, &c.........................	50
G. S. Johnson, for an Investigation into the Nature of the Bases (organic) in the Juice of Flesh.....................	50
S. U. Pickering, for a full Investigation of the Nature of the Reaction taking place when Solutions are diluted with Water	100
A Committee of the Royal Society, for the Determination of the Relation between the Forces of Gravity at the Kew Observatory, and at the Royal Observatory at Greenwich	150
C. Davison, for the Observation and Recording of Earthquakes and Earth-tremors in the Midland Counties..........	80
E. Nevill, for continuing his Investigation of the Errors of Hansen's Lunar Tables	50
P. G. Tait, for a Research on the Duration of Impact and the Coefficient of Restitution	40
G. J. Symons, for completing the Construction of Recording Apparatus for the Study of the Barometric Oscillations which occur during Thunderstorms............................	25
Carried forward.................	£2,675

Brought forward................£2,675

G. J. Symons, for completing the Collection of Records of British Rainfall during the 17th and 18th Centuries 50

P. F. Frankland, for Payment of an Assistant in a Research on the Chemical Changes brought about by Micro-organisms.. 50

A. J. Herbertson and A. Rankin, for obtaining Photographs of Phenomena seen at Ben Nevis Observatory 25

Dr. Armstrong, for a Committee, for a Determination for certain solutions of the Variation in Electrical Resistance with concentration at different Temperatures.................... 150

H. B. Dixon, for a Research on the Rate of Explosion of Cyanogen, Marsh-gas, Ethylene and Acetylene, with Oxygen and Diluents, and two other Researches................... 100

C. R. Alder Wright, for a Research on certain Alloys...... 50

C. A. Ballance and S. G. Shattock, for a Research on the Pathology of Cancer 50

Joseph Thomson, for an Expedition to the Atlas Mountains and the Southern Provinces of Morocco 100

A. C. Haddon, for an Investigation of the Fauna, Structure, and Mode of Formation of the Coral Reefs in Torres Straits.. 300

J. Beard, for Researches on Comparative Vertebrate Morphology, and especially on Ganoid Development 200

T. W. Bridge, for Investigating the Structure of the Airbladder in certain Teleostean Fishes 25

A Committee, for continuation of Mr. Rattray's Monograph of the Diatomaceæ 100

R. Kidston, for continuation of his Investigations into the Distribution of the Carboniferous Flora.................... 40

West Indies Fauna and Flora Committee, for further aid in sending a Collector to obtain Botanical and Zoological Specimens in the less known West Indian Islands................ 100

E. A. Schäfer, for further payment of an Assistant to aid in prosecuting a Research into the functions of the Nervous System, especially of the Cerebral Cortex 50

W. F. Denning, for further observation of Shooting Stars and their Radiant Points 30

W. K. Parker, for continuation of Researches into the Morphology of the Vertebrata...... 150

T. R. Jones, for further Elucidation of the Fossil Ostracoda 25

W. E. Hoyle, to complete the Anatomical Investigation of the Cephalopoda collected by the "Challenger" 100

£4,370

Dr.	£	s.	d.	*Cr.*	£	s.	d.
To Balance, November 30, 1887 .	22	5	6	By Appropriations, as above	4,870	0	0
„ Grant from Treasury	4,000	0	0	Salaries, Printing, Postage, Advertising, and other Administrative Expenses	88	18	0
„ Repayments	617	0	6	By Balance, Nov. 30, 1888	209	19	11
„ Interest on Deposit.........	29	11	11				
	£4,668	17	11		£4,668	17	11

Account of Grants from the Donation Fund in 1887–88.

	£	s.	d.
Prof. T. R. Jones, for illustrations of his work on the Fossil Astracoda, £25. On account	6	18	6
Prof. Schäfer, to assist the investigation by Drs. Martin and Dawson Williams, on the Influence of Bile upon the Digestive Actions of the Pancreatic Juice	20	0	0
Prof. Lankester, to assist in constructing apparatus for the study of certain Medusæ..........................	15	0	0
J. N. Langley, to assist Dr. Sherrington in researches on Histological Changes in the Nervous System	20	0	0
	£61	18	6

Report of the Kew Committee for the Year ending October 31, 1888.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is constituted as follows:

Mr. Warren de la Rue, *Chairman.*

Captain W. de W. Abney, C.B., R.E.
Prof. W. G. Adams.
Staff-Commander E. W. Creak, R.N.
Prof. G. C. Foster.
Mr. F. Galton.
Admiral Sir G. H. Richards, K.C.B.
The Earl of Rosse.
Mr. R. H. Scott.
Lieutenant-General R. Strachey, C.S.I.
General J. T. Walker, C.B.

The work at the Observatory may be considered under the following heads:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the above departments.
5th. Verification of instruments.
6th. Rating of Watches and Marine Chronometers.
7th. Miscellaneous.

I. Magnetic Observations.

There have been no changes introduced in the magnetographs during the year, but the building operations referred to later on have involved the introduction of several pieces of iron, in the shape of girders, standards, rails, &c., both temporarily and permanently, into the field of action of the magnets, and will therefore somewhat complicate the corrections necessary to render the observations comparable with those made prior to the alterations. Fortunately the building in which the absolute observations are made is sufficiently remote (about 100 yards) from the main building to be quite unaffected by these sources of magnetic disturbance.

The photographed magnetic curves representing Declination,

Horizontal Force, and Vertical Force variations have been secured uninterruptedly throughout the past year, and in accordance with the usual practice the scale values of all the instruments were determined in January last.

The following values of the ordinates of the different photographic curves were then found:—

Declination: 1 inch=0° 22′·04. 1 cm.=0° 8′·7.

Bifilar, January 12, 1888, for 1 inch δH = 0·0279 foot grain unit.
„ 1 cm. „ = 0·00051 C.G.S. unit.
Balance, January 16, 1888 „ 1 inch δV = 0·0282 foot grain unit.
„ 1 cm. „ = 0·00051 C.G.S. unit.

The distance between the dots of light upon the vertical force cylinder having become too small for satisfactory registration, the instrument was re-adjusted for balance. This was done on January 19th, after which the scale value was re-determined with the following result:—

Balance, January 21, 1888, for 1 inch δV = 0·0278 foot grain unit.
„ 1 cm. „ = 0·00050 C.G.S. unit.

In February experiments were undertaken to verify the temperature corrections of the force magnetographs as well as of the barograph by artificially heating the room in which these instruments are at work. A rough temporary fireplace was built of bricks and slates, in which a charcoal fire was lighted for several hours. This was subsequently extinguished and the windows were thrown wide open in order to admit the cold night air for a corresponding period. By this means changes of temperature of about 20° F. were several times made. The resultant effect in the case of the bifilar was very small indeed, but with respect to the balance magnetometer, it was considerable, as expected.

In order to ascertain whether the experiments had affected the permanent magnetism of the needles, or had otherwise influenced the instruments, scale value determinations were made on March 20th, and as will be seen by the following note, no appreciable effect had been produced in the sensibility of the V.F. magnetometer by the operation.

Balance, March 20, 1888, for 1 inch δV = 0·0277 foot grain unit.
„ 1 cm. „ = 0·00050 C.G.S. „

Small unimportant repairs have been made to the recording apparatus when necessary.

Although the magnets generally have been more active than in the preceding year, no very large movements have been registered.

The principal disturbances were recorded on the following dates:

November 21, 1887, January 28, April 11–12, May 21, August 3, and October 19–22, 1888.

The monthly observations with the absolute instruments have been made as usual, and the results are given in the tables forming Appendix I of this Report.

The following is a summary of the number of magnetic observations made during the year:—

Determinations of	Horizontal Intensity........	36
„	Inclination................	124
„	Absolute Declination........	39

The magnetograph curves made use of in the preparation of the tables of diurnal range of Declination (see Appendix I, Table III) have been reproduced from the original photographs by means of an eidograph kindly lent by Captain Wharton, F.R.S., the Hydrographer.

A complete set of self-recording magnetographs by Casella, London, similar in construction to the instruments recently supplied to the Royal Cornwall Polytechnic Society, have been examined at the Observatory.

Information on matters relating to terrestrial magnetism and various data have been supplied to Professors Rücker, Piazzi Smyth, Dr. Rijckevorsel, and Messrs. Wilkinson and Harrison.

Magnetic Reductions.—At the request of the Rev. S. J. Perry, copies of the Kew Horizontal Force curves for certain selected days during the years 1883 to 1886 are now being made.

II. Meteorological Observations.

The several self-recording instruments for the continuous registration respectively of Atmospheric Pressure, Temperature, and Humidity, Wind (direction and velocity), Bright Sunshine, and Rain, have been maintained in regular operation throughout the year.

The standard eye observations for the control of the automatic records have been duly registered, together with the daily observations in connexion with the U.S. Signal Service synchronous system. A summary of these observations is given in Appendix II.

The tabulation of the meteorological traces has been regularly carried on, and copies of these, as well as of the eye observations, with notes of weather, cloud, and sunshine have been transmitted to the Meteorological Office.

Owing to trouble caused by bursting of the water-reservoir for the thermograph wet-bulbs during frosty weather, and the risk of their imperfect action owing to leakage of water, a double tank has been made, so that in the event of the inner vessel bursting, the outer one will prevent any loss of water.

The number of instruments under observation has been increased by the addition of a snow gauge on Professor Nipher's principle for the purpose of measuring falls of snow, but no opportunity has occurred since its erection of thoroughly testing its indications.

A new 8-inch Glaisher gauge has been supplied by the Meteorological Office, and its readings observed regularly, since January, with the view of substituting it for the old square 100-inch area gauge hitherto employed for check upon the Beckley S.R. gauge, on the completion of a full year's comparison of the two gauges.

Seven months' observations have also been made of a second 8-inch gauge, with the view of determining the effect of paint upon the inner surface of the collecting funnel.

During the period that the east room of the Observatory was undergoing alteration, the working standard barometer, Newman 34, was temporarily removed to a position a few yards distant in the North Hall. Comparisons were made with the Welsh standards (which were carefully cased in, during the time of occupation of the room by workmen), both before, subsequent to its removal, and after its replacement in its old position.

The following is a summary of the number of meteorological observations made during the past year:—

Readings of standard barometer		1740
,,	dry and wet thermometers	3480
,,	maximum and minimum thermometers	732
,,	radiation thermometers	1285
,,	rain gauges	1532
Cloud and weather observations		1882
Measurements of barograph curves		8764
,,	dry bulb thermograph curves	9462
,,	wet bulb thermograph curves	8668
,,	wind (direction and velocity)	17472
,,	rainfall curves	795
,,	sunshine traces	1891

In compliance with a request made by the Meteorological Council to the Committee, Mr. Whipple visited and inspected during his vacation the Observatories at Aberdeen, Glasgow, Stonyhurst, and Oxford, as well as the anemographs at Swanbister, North Shields and Fleetwood.

Mr. Baker also inspected the Falmouth and Valencia Observatories as well as the Anemographs at Mountjoy Barracks (Dublin) and Holyhead.

Advantage was taken of these visits to fit Stonyhurst lifters to

the Beckley rain gauges at Aberdeen, Falmouth, and Valencia, and one has since been forwarded to Dr. Dreyer for him to fit at Armagh.

The barograph and thermograph formerly in use at the Armagh Observatory, after being put in thorough repair, have been erected in the Verification-house and temporarily set to work, awaiting the decision of the Meteorological Council as to their final disposition.

With the sanction of the Meteorological Council, weekly abstracts of the meteorological results have been regularly forwarded to, and published by 'The Times' and 'The Torquay Directory.' Data have also been supplied to the Council of the Royal Meteorological Society, the editor of 'Symons's Monthly Meteorological Magazine,' the Secretary of the Institute of Mining Engineers, Captain Abney, Dr. Rowland, and others. The cost of these abstracts is borne by the recipients.

Since January last tables of the monthly values of the rainfall and temperature have been prepared and sent to the Meteorological Sub-Committee of the Croydon Microscopical and Natural History Club for publication in their Proceedings. Detailed information of all thunderstorms observed in the neighbourhood during the year has also been regularly forwarded to the Royal Meteorological Society soon after their occurrence, special forms having been provided by the Society for the purpose of collecting such information with the view to special investigation.

Electrograph.—The electrograph under repair at time of last Report, owing to its partial destruction by fire, has been put in thorough order. The de la Rue battery, employed to charge it, has been cleaned, and its cells refilled by the makers. The scale-value of the instrument has been again determined by means of the portable electrometer (White's) and found to be practically unaffected by the accident.

A paper giving a summary of the results afforded by the instrument is at present in preparation.

The electrometer lent to Mr. Abercromby for the purpose of making observations during his expedition to Teneriffe was returned to the Observatory in good order on the termination of his experiment, and on trial the value of the scale division was found to be unaltered.

In consequence of an accident whilst cleaning, the instrument required re-adjustment in March, but no alteration was found to have resulted to its sensitiveness when again tested at the laboratory in Charlotte Street, facilities being afforded for this by the kindness of the Chairman.

III. Solar Observations.

The sketches of Sun-spots, as seen projected on the photoheliograph screen, have been made on 150 days, in order to continue Schwabe's enumeration, the results being given in Appendix II, Table IV.

Transit Observations.—Regular observations of solar and of sidereal transits have been taken, for the purpose of keeping correct local time at the Observatory, and the clocks and chronometers have been compared daily.

The clocks, French, Shelton K. O., Shelton 35, and the chronometers Breguet No. 3140, and Arnold 86 are kept carefully rated as time-keepers at the Observatory, and the mean-time clock, Dent 2011, lent by the Astronomer-Royal, is also in use in daily comparisons with the chronometers on trial.

The chronometer, Molyneux No. 2126, is used as a "hack chronometer" in order to facilitate the inter-comparison of the clocks.

The scale, figures, &c., on the south meridian mark in connexion with the transit-instrument having become somewhat obliterated through age and exposure, steps were taken to remedy this defect, and some slight improvements introduced.

IV. Experimental Work.

Photo-nephograph.—The past year has been particularly unfavourable to cloud photography at the Observatory.

The opportunities of taking negatives of cirrus, to which particular attention is directed, were rare in the earlier months of the summer, and during the later the builders' operations prevented, in a great measure, the work being carried on.

Several modifications have been introduced into the system of observing, materially simplifying it, and the mathematical treatment of the pictures has also been temporarily set aside in favour of mechanical methods, which afford results of sufficient accuracy in a small fraction of the time occupied by the other plans of reduction which have been employed hitherto.

Observations of cloud height, drift, and direction have been treated in this manner for 1887 and for 1888, generally with satisfactory results. During April special photographs were taken with one camera only, for showing the structural change in cirrus in short intervals of time, and seven sets of negatives were procured, exhibiting the extensive alteration sometimes observed in this class of cloud in a couple of minutes.

Time Signals.—With a view of obtaining the time at the Observatory for pendulum work to a high degree of accuracy, and also for comparing daily the time as determined by the Observatory Transit with that distributed by the Postmaster-General from St. Martin's-le-

Grand, application was made to H.M. Commissioners of Woods and Forests for permission to erect a telegraph line from the Observatory to the London and South Western Railway skirting the Old Deer Park. This was granted, and a line has been set up placing the Observatory in direct electrical communication with the Chief Post Office in Richmond.

A relay and chronograph have been purchased and placed in the circuit, and every morning, excepting Sundays and holidays, the 10 A.M. signal from the Royal Observatory, Greenwich, is recorded beside the beats of the Observatory Standard Clock (French) on the same tape. The signals have been observed daily by means of the galvanometer for the past two months, but the chronograph was only regularly set to work on the 31st October, delay having arisen on account of the necessity of protecting the apparatus against lightning.

The cost of the chronograph and attachments to the Standard Clock has been defrayed by a grant from the Royal Society.

Pendulum Experiments.—The swinging of the Indian Invariable Pendulums at the Observatory has been delayed by the operations attendant on the establishment of the time signal connexion with the General Post Office, and also by failure, up to the present, of information from the American officers as to certain details of their practice when observing with the apparatus in America and elsewhere.

Meanwhile experiments have been made to determine the vacuum correction of the two thermometers, Nos. K.S. 667 and 668, used on the dummy to replace those broken in travelling. It was observed that a reduction of 27 inches in the barometric pressure lowered their zero points by 0·25°. Other observations were also made to find the relative degree of accordance during changes of temperature between the indications of the thermometers in the interior of the vacuum-chamber and that attached to the Richard thermograph placed in close proximity to its outer surface.

During these trials the holding capacity of the chamber has been thoroughly tested and found to stand low pressures extremely well.

Constants of Robinson Anemometers.—By permission of the Committee, Mr. Whipple has attended at Hersham on several occasions, and assisted Mr. W. Dines, B.A., F.R. Met. Soc., in the experiments in progress, on behalf of the Wind Force Committee of the Royal Meteorological Society, for determining the value of the Robinson constant for anemometers of various dimensions, and also for verifying the factor for converting wind velocity into pressure.

The experiments are similar to those carried out at the Crystal Palace in 1874, and described in the Report for that year.

A Preliminary Report on the experiments was read before the R. Met. Soc. meeting in May, 1888, and is printed in the 'Quarterly Journal,' vol. 14, p. 253. The results compare very favourably with

those formerly obtained as discussed by Professor Stokes ('Roy. Soc. Proc.,' vol. 32, p. 170).

V. Verification of Instruments.

The following magnetic instruments have been purchased on commission and their constants determined:—

An Inclinometer for the Tiflis Observatory.
A pair of Inclinometer Needles for the Colaba Observatory.
Ditto for the U.S. Navy Department.
Ditto for the Utrecht Observatory.

The total number of other instruments compared in the past year was as follows:—

Air-meters	6
Anemometers	2
Aneroids	164
Barometers, Marine	31
,, Standard	75
,, Station	9
Compasses	7
Hydrometers	543
Inclinometers	1
Magnets	3
Rain Gauges	3
Sextants	157
,, Shades	78
Sunshine Recorders	3
Theodolites	3
Thermometers, Arctic	136
,, Avitreous or Immisch's	1591
,, Chemical	79
,, Clinical	10442
,, Deep sea	77
,, Meteorological	1074
,, Mountain	27
,, Solar radiation	3
,, Standards	73
Unifilars	1
Total	14,588

Duplicate copies of corrections have been supplied in 52 cases.

The number of instruments rejected on account of excessive

error, or which from other causes did not record with sufficient accuracy, was as follows:—

Thermometers, clinical	51
" ordinary meteorological	16
Various	221

7 Standard Thermometers have also been calibrated, and supplied to 2 societies and 2 individuals during the year.

There are at present in the Observatory undergoing verification, 22 Barometers, 482 Thermometers, 2 Hydrometers, and 4 Sextants.

Sextant Verification.—The number of sextants submitted for examination continues to increase, having amounted during the past year to 157.

VI. Rating of Watches.

639 entries of watches for rating were made as contrasted with 510 during the corresponding period of last year. They were sent for testing in the following classes:—

For class A, 569; class B, 51; and class C, 19.

Of these 218 failed to gain any certificate; 8 passed in C, 46 in B, 367 in A, and 28 of the latter obtained the highest possible form of certificate, the class A *especially good.*

In Appendix III will be found statements giving the results of trial of the 30 watches which obtained the highest numbers of marks during the year, the premier position being attained—with 89·0 marks—by a keyless, single-roller, going-barrel, centre-seconds watch, submitted by W. Holland, Rockferry, Birkenhead.

This total exceeds that of last year, and it is also extremely satisfactory to note that a continued increase has taken place in the proportion of watches gaining more than 80 marks, the number this year being 53.

No difficulty has been experienced in maintaining the three safes—in which the watches are placed during rating—at the three temperatures of 40°, 65°, and 90° respectively, all the year round.

Special attention continues to be given, as before, to the examination of *pocket chronographs*, in accordance with the request of the Cyclists' Union; and in consequence of numerous enquiries from manufacturers, timers, &c., a set of rules has been drawn up, as follows, which are adhered to as far as practicable in testing chronographs.

1. After the usual A or B tests are finished the watch is run with the chronograph work in continual action for one or two periods of 24 hours each, and a note made of the maximum effect produced upon the ordinary daily rate, by the chronograph mechanism being in constant action.

2. This maximum effect must not exceed $\pm$ 5 seconds.

3. In addition to the above 24-hour trials, the watch—with a view of testing its starting, stopping, and recording qualifications—is also subjected to shorter tests, varying from a few seconds to an hour or more in duration.

4. When the chronograph mechanism is in action, and pressure is applied to the knob or push-piece, the chronograph hand or hands must either stop dead at once, or else must run on unaffected until stronger force is used.

5. There must be a complete absence of "lagging," and moving only in spasmodic jumps, when pressure is applied, and perfect absence of recoil when the chronograph hand is stopped.

6. The hands must return to, and start exactly from, the zero mark, and in the case of split seconds they must run together in exact accordance.

7. The times shown by the minute-recorder must agree with the position of the fly-back hand.

8. When the chronograph action of a watch has been tested—in addition to the trial of its ordinary time-keeping qualities—an endorsement of the result will be made upon the certificate; and chronograph watches with certificates without this endorsement will be recognised as having been examined as ordinary watches only.

Marine Chronometers.—Certificates of mean daily rate and of variations of rate at each temperature have been awarded to 12 marine chronometers after undergoing the 35 days' trial as specified in the regulations.

VII. Miscellaneous.

Assistance to Observatories, &c.—Prepared photographic paper has been procured and supplied to the Observatories at Batavia, Colába, Falmouth, Lisbon, Mauritius, Oxford, St. Petersburg, Stonyhurst, and Toronto, as well as also to the Meteorological Office and the U.S. Navy Department.

Anemograph sheets have likewise been sent to Coimbra and Mauritius, blank forms for entry of observations, &c., have also been distributed to various applicants.

Hongkong Observatory.—This observatory was founded by H.M. Government in 1883, partly on the recommendation of the Kew Committee, in order amongst other objects to obtain continuous observations of terrestrial magnetism and meteorology in the eastern hemisphere between Java and Zi-Ka-Wei.

The Committee have recently been consulted by the Secretary of State for the Colonies as to the advisability of suspending the magnetic work of the Chinese Observatory for a period of three years,

but having regard to the important changes going on in the horizontal component of the earth's magnetism, on that part of the globe, they were not able to recommend the Secretary to interrupt the observations as suggested.

Marine Telescopes.—The arrangements described in last year's Report for the examination of Marine telescopes and binoculars have been completed, and a circular has been approved of by the Committee for issue to the public, stating that such instruments will in future be tested at Kew on payment of the following fees :—

Marine telescopes and superior binoculars	..	2*s.* 6*d.* each.
Opera glasses and pocket telescopes ..	..	1*s.* 6*d.* „

The Secretary of the Admiralty has communicated with the Com mittee with reference to a proposal that all such instruments purchased for use in H.M. Navy should be examined at the Observatory prior to their acceptance from the contractors' hands.

Photographic Lenses.—Captain Abney, at the suggestion of the Camera Club, as well as Mr. Galton, have proposed to the Committee the establishment of a system of testing and certifying lenses constructed for use in photographic cameras. Captain Abney has proposed a scheme of examination, and experiments are in progress with a view to carrying it out at the Observatory. It has, however, been found difficult as yet to fix upon one which would permit of a sufficiently exhaustive examination being conducted for the low fee which has been suggested, as probably the only one likely to make the certificates popular.

Ships' Lights.—The Committee have had under consideration the very important subject of the examination of ship's lights for the Mercantile Marine, by a system based upon the method now in operation at H.M. Dockyard at Chatham with reference to the lamps, lenses, and coloured shades used in H.M. Navy.

The inland isolated position of the Observatory, and the heavy and cumbersome nature of the lanterns, appear to the Committee at present to offer an almost insuperable objection to the adoption of this at Kew. There are no funds available for the alternative plan suggested of setting up a branch establishment at some locality on the banks of the Thames below London.

Exhibition.—The Committee contributed to the Annual Exhibition of the Royal Meteorological Society held in March last, a collection of apparatus for observing atmospheric electricity, principally that used at Kew by Ronalds or subsequent observers.

A list of the various objects with references is printed in the catalogue prepared by the Society.*

* See 'Quarterly Journal,' vol. 14, p. 235.

Extension of the Building.—The Chief Commissioner of Works and Public Buildings having granted permission for the Committee to undertake the erection of the additional storey to the east wing of the Observatory as mentioned in last year's Report, and having instructed Mr. Lessels, surveyor to the Board, to prepare the necessary drawings, plans, &c., tenders were invited from the principal local builders for the work. That of Messrs. J. Dorey and Co., of Brentford, for £454, was accepted, and operations were commenced on July 23rd. They have now been completed under the superintendence of Mr. Chart, H.M. Commissioners' Clerk of Works for the Hampton Court and Kew District, and Mr. Allen, his Assistant.

Library.—During the year the library has received as presents the publications of—

22 Scientific Societies and Institutions of Great Britain and Ireland, and

95 Foreign and Colonial Scientific Establishments, as well as numerous private individuals;

The reference set of 'Phil. Trans.' has been bound in cloth boards to correspond with the covers of the volumes as now issued by the Royal Society.

Old Mural Quadrant.—When in 1840 the astronomical instruments forming the equipment of George III's Observatory, were removed to Armagh, it was found impracticable to take away the 8-feet mural quadrant by Sissons, on account of its being too large to pass through the doors or windows of the room in which it was placed.

Recently, advantage was taken of the removal of the roof of the east wing of the Observatory to hoist it out and convey it to the Stores in the Office of Works at Kew, where it is now deposited. The Committee propose its ultimate consignment to the Loan Collection of Scientific Apparatus at South Kensington.

The stone wall which served for its support has been utilised as a bearer for a new gallery, providing an additional area of 29 feet long by 7 feet wide, which it is intended to devote to the Department for the Verification of Hydrometers.

Workshop.—The machine tools procured for the use of the Kew Observatory by grants from the Government Grant Fund or the Donation Fund, have been duly kept in order.

House, Grounds, and Footpath.—These have all been kept as usual during the year.

A Norton's tube-well has been driven and a pump erected in order to obtain an increased water supply, the Observatory not being in connexion with the mains of Richmond.

PERSONAL ESTABLISHMENT.

The staff employed is as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, Chief Assistant.
H. McLaughlin, Librarian.
E. G. Constable, Observations and Rating.
W. Hugo, Verification Department.
J. Foster " "
T. Gunter.
W. J. Boxall, and five other Assistants.

The Committee feel that they cannot permit the lamented death of Professor Balfour Stewart to pass unnoticed.

Professor Stewart's connexion with the Observatory originated in 1856, when it was under the control of the British Association. In February of that year he joined the staff as an Assistant Observer to Mr. John Welsh; his stay was, however, short, as he left soon after in October to become Assistant to Professor Forbes at Edinburgh, but returned in 1859 as the Superintendent, accepting the appointment when offered him on the death of Mr. Welsh. He relinquished the superintendence in 1871, in order to reside at Manchester as Professor of Physics in Owens College, but maintained a most lively interest in the operations of the Observatory, especially in the solar and magnetic work, being engaged in a discussion of certain of the Kew magnetic observations even up to the time of his death. The most important of his papers referring to these and similar observations are enumerated in the appendix to Mr. Scott's "History of the Kew Observatory."*

(Signed) WARREN DE LA RUE, *Chairman.*

November 27th, 1888.

* See 'Roy. Soc. Proc.,' vol. 39, pp. 37–86 (1886).

The Kew Observatory. Account of Receipts and Payments for the year ending November 3rd, 1883.

Dr. RECEIPTS.

	£	s.	d.	£	s.	d.
To Balance from 1886–87	£775	11	8			
Royal Society (Gassiot Trust)	492	10	9			
„ (Pendulum)	250	0	0			
Meteorological Office (Allowance)	400	0	0			
Experimental Work	67	1	8			
Miscellaneous	64	15	8			
Verifications	890	2	7			
Rating of Watches and Chronometers	532	1	11			
				3492	4	8
Meteorological Office for Postages and Porterages				5	16	3
Commissions executed for Colonial and Foreign Institutions, &c.				291	19	9
				£3790	0	8

PAYMENTS. **Cr.**

	£	s.	d.	£	s.	d.
By Salaries and Extra	£1596	15	7			
Gas, Fuel, Rent, Insurance, &c	136	8	3			
Stationery, Postages, Porterages, &c	70	17	0			
Instruments, Apparatus, &c.	45	4	9			
Experimental Work	32	6	5			
Miscellaneous	43	13	10			
Exhibition	2	13	9			
Verifications	130	1	10			
Rating of Watches and Chronometers	106	6	1			
Pendulum	79	17	3			
				2244	4	8
Extension of the Building				405	0	0
Postages and Porterages, &c., for Meteorological Office				32	3	2
Commissions executed for Colonial and Foreign Institutions, &c.				270	5	7
Balance—Bank of England	747	6	2			
London and County Bank	71	10	10			
Cash in hand	19	9	10			
				838	6	10
				£3790	0	8

November 22, 1888. Examined and compared with the Vouchers, and found correct. (Signed) W. G. ADAMS, *Auditor.*

ASSETS.

	£	s.	d.
By Balance as per Statement	838	6	10
Meteorological Office Allowance, Experimental, and Sundries	106	13	7
Verification Fees due, &c.	68	0	5
Rating of Watches, &c.	64	1	4
Photographic Paper	6	0	9
Commissions, &c.	19	8	8
Blank Forms	23	5	3
Standard Thermometers in Stock	80	14	0
	£1206	10	10

LIABILITIES.

	£	s.	d.
To Gas, Fuel, and House Account	24	3	7
Building Extension and Painting	100	0	0
Pendulum Account—Unspent Balance	210	12	10
„ „ Outstanding Account	3	0	0
Chemicals, &c.	19	5	5
Purchases on Commission	34	19	0
Balance	814	10	0
	£1206	10	10

November 22, 1888. (Signed) G. M. WHIPPLE, *Superintendent.*

APPENDIX I.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N. Long.* $0^h\ 1^m\ 15^s{\cdot}1$ *W., for the year October* 1887 *to September* 1888.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones.

The Declination observations have also been made with the same Magnetometer, Collimator Magnet N E being employed for the purpose.

The Dip observations were made with Dip-circle Barrow No. 33, the needles 1 and 2 only being used; these are $3\frac{1}{2}$ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the unit in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the *factor for reducing the English to metric values being* 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log π^2K employed in the reduction is 1·64365 at temperature 60° F.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° F. is

$$0{\cdot}0001194(t_0-35)+0{\cdot}000,000,213(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 foot and 1·3 feet, are 1·000075 foot and 1·300097 feet respectively.

The times of vibration given in the Table are each derived from the mean of 14 observations of the time occupied by the magnet in making 100 vibrations, corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^{\,3}}\right)$, is −0·00168.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected 1,250 feet north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, has been carefully determined.

The observations have been made and reduced by Mr. T. W. Baker.

Table I.

Observations of Inclination or Dip.

Month.	Mean Inclination.	Month.	Mean Inclination.
1887.		1888.	
October 25......	67° 37′·9	April 23......	67° 35′·5
26......	67 39·1	24.....	67 35·3
		25......	67 36·0
Mean......	67 38·5	Mean......	67 35·6
November 28......	67 35·8	May 22......	67 37·0
29......	67 38·2	23......	67 36·2
		24......	67 37·1
Mean......	67 37·0	Mean.....	67 36·8
December 28......	67 39·3	June 26......	67 33·1
29......	67 36·7	28......	67 34·9
Mean......	67 38·0	Mean......	67 34·0
1888.			
January 25......	67 37·3	July 24......	67 35·7
27......	67 36·3	25......	67 34·2
28......	67 36·5	26......	67 35·2
Mean......	67 36·7	Mean......	67 35·0
February 23......	67 37·2	August 27......	67 35·8
24......	67 37·1	29......	67 35·6
Mean......	67 37·1	Mean......	67 35·7
March 23......	67 36·6	September 24.....	67 35·4
27......	67 36·6	26.....	67 35·7
Mean......	67 36·6	Mean......	67 35·6

Table II.

Observations for the Absolute Measurement of Horizontal Force.

Month.	Log $\frac{m}{X}$ mean.	Log mX mean.	Value of m*.
1887.			
October 27th	9·12043	0·30726	0·51743
November 30th	9·12030	0·30776	0·51764
December 30th...... ..	9·12012	0·30796	0·51765
1888			
January 30th.....	9·11995	0·30803	0·51760
February 28th	9·12015	0·30813	0·51777
March 29th	9·11981	0·30826	0·51764
April 26th	9·11989	0·30817	0·51764
May 25th and 26th	9·11960	0·30832	0·51756
June 30th	9·11976	0·30859	0·51782
July 30th	9·12008	0·30840	0·51780
August 28th	9·11986	0·30823	0·51767
September 26th	9·12022	0·30793	0·51770

Table III.—Solar Diurnal Range of the Kew Declination as derived from selected quiescent days.

Hour	Summer mean.	Winter mean.	Annual mean.
1888.			
Midnight	−0′·7	−0′·7	−0′·7
1	−0·6	−0·6	−0·6
2	−0·8	−0·3	−0·5
3	−1·1	−0·5	−0·8
4	−2·0	−0·2	−1·1
5	−2·6	−0·1	−1·4
6	−3·4	−0·3	−1·8
7	3·9	−0·7	−2·3
8	−4·0	−1·3	−2·6
9	−3·5	−1·3	−2·4
10	−1·0	−0·8	−0·9
11	+1·8	−0·2	+0·8
Noon	+4·2	+1·3	+2·8
13	+5·9	+2·8	+4·4
14	+5·4	+2·0	+3·7
15	+4·3	+1·2	+2·8
16	+2·7	+0·6	+1·7
17	+1·2	+0·3	+0·8
18	+0·1	−0·2	0·0
19	−0·1	−0·7	−0·4
20	−0·3	−0·7	−0·5
21	−0·4	−0·8	−0·6
22	−0·6	−1·0	−0·8
23	−1·1	−1·0	−1·0

* m = magnetic moment of vibrating magnet.

Table IV.

Month.	Declination	Magnetic Intensity								
		English Units.			Metric Units			C. G. S. Measure		
	Mean of Observations.	X, or Horizontal Force.	Y, or Vertical Force	Total Force.	X, or Horizontal Force	Y, or Vertical Force	Total Force.	X, or Horizontal Force	Y, or Vertical Force.	Total Force.
	West									
1887 October	18° 13′ 52″	3·9211	9·5330	10·3079	1·8117	4·3955	4·7528	0·1812	0·4396	0·4753
November	18 12 58	3·9240	9·5282	10·3046	1·8093	4·3933	4·7513	0·1809	0·4393	0·4751
December	18 9 26	3·9257	9·5404	10·3164	1·8101	4·3989	4·7567	0·1810	0·4399	0·4757
1888. January	18 12 18	3·9268	9·5326	10·3098	1·8106	4·3953	4·7537	0·1811	0·4395	0·4754
February......	18 7 36	3·9264	9·5348	10·3115	1·8104	4·3963	4·7544	0·1810	0·4396	0·4754
March........	18 7 51	3·9234	9·5359	10·3134	1·8113	4·3968	4·7553	0·1811	0·4397	0·4755
April	18 8 1	3·9277	9·5262	10·3041	1·8110	4·3924	4·7511	0·1811	0·4392	0·4751
May..........	18 7 16	3·9297	9·5405	10·3181	1·8119	4·3990	4·7575	0·1812	0·4399	0·4758
June	18 8 27	3·9302	9·5198	10·2991	1·8122	4·3894	4·7488	0·1812	0·4389	0·4749
July..........	18 8 27	3·9279	9·5218	10·3003	1·8111	4·3904	4·7493	0·1811	0·4390	0·4749
August	18 7 4	3·9281	9·5280	10·3058	1·8112	4·3932	4·7518	0·1811	0·4393	0·4752
September	18 5 4	3·9252	9·5201	10·2975	1·8098	4·3896	4·7480	0·1810	0·4390	0·4748

APPENDIX II.

Meteorological Observations.—Table I.

Mean Monthly results.

Months.	Thermometer.								Barometer.*					Mean vapour-tension.
	Mean.	Means of—			Absolute Extremes.				Mean.	Absolute Extremes.				
		Max.	Min.	Max. and Min.	Max.	Date.	Min.	Date.		Max.	Date.	Min.	Date.	
1887.	°	°	°	°	°	d. h.	°	d. h.	ins.	ins.	d. h.	ins.	d. h.	in.
Oct.	44·8	51·8	38·6	45·2	60·3	8 2 P.M.	26·4	26 {7 & 8 A.M.}	30·104	30·623	18 9 A.M.	28·844	30 5 A.M.	·235
Nov.	41·0	45·1	36·4	40·8	53·8	4 1 „	23·2	16 7 „	29·716	30·419	16 10 „	28·796	4 3 „	·221
Dec.	38·2	42 2	34·0	38·1	54·6	9 1 A.M.	25·5	27 7 „	29 869	30·504	2 10 „	29·275	15 {4 & 5 A.M.}	·197
1888.														
Jan.	38·1	41·8	34·0	37·9	51·1	8 6 P.M.	24·2	30 8 „	30·250	30·743	10 11 „	29·245	31 5 P.M.	·198
Feb.	35·6	39·3	32·0	35·7	51·3	6 2 „	21·6	2 8 „	29·972	30·431	28 9 P.M	29·438	1 1 A.M.	·170
March..	38·5	44·0	33·9	39·0	55·3	10 3 „	25·3	2 5 „	29 627	30 337	1 1 „	28·732	28 3 P.M.	·188
April...	43·6	50·7	37·4	44·1	64·5	15 2 „	28·3	6 3 „	29·902	30 289	6 Midt.	29·469	30 Midt.	·219
May	52·3	61·1	43·7	52·4	73·8	19 3 „	34·3	12 5 „	30·065	30·499	11 8 A.M.	29·411	1 9 A.M.	·278
June	57·5	65·5	50·5	58·0	82·0	25 3 „	44·4	17 Midt.	29·938	30·266	1 10 P M	29·538	29 4 P.M.	·365
July	57·9	64·6	52·0	58·3	71·3	22 3 „	43 6	11 8 A M.	29·779	30·144	13 {6, 7, & 9 A.M.}	29·394	28 {4 & 5 A.M.}	·394
Aug.	58·5	66·2	51·1	58·7	79·7	10 1 „	43·4	19 5 „	30·018	30·343	31 Midt.	29·500	28 9 P.M.	·392
Sept.	55·4	62·8	48·7	55·8	71·0	15 4 „	39·2	30 11 P.M.	30·156	30 518	12 10 P M.	29·562	29 11 „	·368
Means..	46·8	52·9	41·0	47·0	..	....	..	....	29·950	..	....	..	....	·269

The above Table is extracted from the " Hourly Readings," vols. 1887–88, of the Meteorological Office, by permission of the Meteorological Council.

* Reduced to 32° at M.S.L.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall*.			Weather. Number of days on which were registered								Wind †. Number of days on which it was								
		Total.	Maximum.	Date.	Rain.	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	Calms.	N.	N.E	E.	S.E	S.	S.W.	W.	N.W.	Variable.
1887.		in.	in.																		
October ..	7	1·465	0·650	29	12	1	2	..	4	14	..	9	10	2	..	..	2	5	4	7	1
November	7	3·050	0·510	3	20	1	1	..	4	14	1	3	4	8	6	..	3	5	4	..	..
December	7	1·360	0·265	14	15	3	1	..	3	16	..	2	7	5	..	1	3	4	9	2	..
1888.																					
January ..	7	0·865	0·270	20	12	2	1	..	3	17	1	5	2	3	3	1	3	7	7	4	1
February .	8	0·905	0·500	13	12	7	..	..	..	23	..	..	6	11	..	..	..	3	5	3	1
March ...	8	3·050	0·420	26	19	4	1	1	..	21	3	..	6	5	1	1	2	7	3	6	..
April	7	2·215	0·680	19	15	2	2	2	1	15	..	..	5	7	3	..	1	8	4	1	1
May	5	1·130	0·380	16	4	..	..	..	8	8	1	2	2	7	3	3	2	6	6	1	1
June	7	2·350	0·515	26	20	..	..	4	1	16	..	2	9	3	2	..	4	6	4	..	2
July	8	4·610	0·500	5	25	..	..	6	1	22	..	3	2	2	2	1	5	11	3	5	..
August ..	6	2·810	0·880	1	12	..	1	2	4	12	..	1	4	2	..	1	5	13	2	3	1
September	6	1·435	0·250	29	13	..	..	..	8	10	..	5	6	7	5	..	1	6	3	2	..
Totals ..		25·245			179	20	9	15	37	188	6	32	63	62	25	8	31	81	54	34	8

* Measured at 10 A.M. daily by gauge 1·75 feet above surface of ground. † As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb *in vacuo*.)			Minimum temperature on the ground.			Horizontal movement of the Air.*		
	Total number of hours recorded.	Mean percentage of possible sunshine.	Greatest daily record.	Date.	Mean	Highest.	Date	Mean.	Lowest.	Date.	Average hourly Velocity	Greatest hourly Velocity.	Date.
1887.	h. m.		h. m		deg.	deg.		deg.	deg.		miles.	miles.	
October	108 6	33	9 6	12	94	113	8	30	15·7	13	8	34	30
November	44 6	16	5 54	30	64†	99	3	32	17·6	16	10	42	1
December	42 36	17	5 18	5	63	82	2	27	18·7	27	9	31	13
1888.													
January	41 0	16	5 12	30	60	85	23	29	15·7	1	11	40	26
February	32 6	11	4 36	1	69	88	13	28	14·1	2	15	34	21
March	58 54	16	9 6	21	86	110	10	30	15·5	2	14	41	11
April	106 24	25	11 30	30	98	123	29	32	18·7	7	12	30	25
May	225 6	46	14 48	23	119	133	31	37	28·1	11	12	39	2
June	132 18	27	13 54	13	113	141	1	46	33·5	18	9	30	12
July	103 30	21	10 48	24	120	134	30	48	36·5	13	10	31	24
August	158 42	35	13 12	14	122	139	9	47	37·3	19	10	27	28
September	126 0	33	10 6	11	110	127	1	44	34·3	11	8	24	9

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.
† Instrument dismounted for two days.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days without spots.
1887.			
October	17	2	10
November	14	2	10
December	12	4	4
1888.			
January	9	4	3
February	6	2	3
March	10	2	4
April	9	5	2
May	19	1	14
June	11	2	3
July	8	1	6
August	16	4	7
September	19	3	6
Totals	150	32	72

APPENDIX III.—Table I.

Results of Watch Trials. Performance of the 31 Watches which obtained the highest number of marks during the year.

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate — +Gaining. —Losing.	Mean variation of daily rate ±	Mean change of rate for 1° F.	Difference of mean daily rate: Between pendant up and dial up.	Difference of mean daily rate: Between pendant up and pendant right.	Difference of mean daily rate: Between pendant up and pendant left.	Difference of mean daily rate: Between dial up and dial down.	Difference between extreme gaining and losing rates.	Marks awarded for: Daily variation of rate	Marks awarded for: Change of rate with change of position.	Marks awarded for: Temperature compensation.	Total Marks. 0—100.
			secs.	secs.	secs.	secs	secs	secs.	secs	secs.				
W. Holland, Rock Ferry	3624†	Single overcoil, *s r., *g.b.	+0.6	0 35	0.02	−1.2	+1.0	+0.5	+0.2	4 0	33.0	37.0	19.0	89.0
H. Golay, London	147106†	Double overcoil, s r., g b.	+3.0	0.5	0.003	−0.8	−2.1	−2.6	−1.0	5.25	29 4	36.6	19.8	85.8
Usher & Cole, London	24329†	Single overcoil, s.r., g.b.	+1 5	0.5	0.04	−0.5	−1.6	−2.1	−0.8	4 75	30.7	37.2	17.6	85.5
E. F. Ashley, London	03614†	Single overcoil, s.r., fusee	+0.7	0.4	0.06	+1 0	−1.8	−0.7	−0 8	4 5	32.0	36.8	15.8	84.6
D. Buckney, London	2930†	Double overcoil, *d r., g.b.	+3 3	0.5	0.05	+0.9	+2.0	+0.4	−2.0	5.75	30.3	36.8	16 9	84.5
Baume & Co., London	2706	Single overcoil, d.r., g.b.	+0.2	0.5	0.04	−1.3	−1.2	−3.0	+0.3	6.25	29.6	37.3	17.5	84.4
Jos. White, Coventry	31610	Single overcoil, s.r., g b.	+0.5	0.6	0.02	−0.6	+0.5	−0.6	+0.5	4.5	27.6	38 6	18.3	84.4
Usher & Cole, London	24042†	Single overcoil, s.r, g.b.	+1.0	0.5	0.04	−0 5	−2 3	−2 6	−0.8	5.25	30.4	36.4	17.3	84.1
Stauffer & Co., London	123175	Single overcoil, d.r., g b., bar-lever	−5.6	0.4	0.05	+2 6	−0.7	−1.5	−2.1	8.0	31 3	35.7	16.8	83.8
Stauffer & Co., London	123162	Single overcoil, d.r., g.b., bar-lever	+5.1	0.5	0.02	−2.8	−4.0	−1.5	+1.5	5.5	29.9	35 3	18.5	83.7
Jos. White, Coventry	31824†	Single overcoil, s.r., g.b.	+1.8	0.5	0 01	+0.7	+1.8	+4.9	+2.8	7.75	30 9	33.6	19.1	83.6
W. Gabriel, London	325/1000	Double overcoil, s.r., g.b.	+1.4	0.5	0.07	−1.0	−0.2	−0.1	+0.4	6.5	29.5	38.7	15.3	83.5
A. E. Fridlander, Coventry	52541†	Double overcoil, d.r, fusee	+0.6	0 4	0.04	+0.1	−2.6	−2 3	−2.6	5.0	31.1	35 1	17.1	83.3
Stauffer & Co., London	123179†	Single overcoil, d r, g.b., bar-lever	−1.1	0.4	0.06	−0.5	−2.5	−3 0	−0.9	6.5	31.2	35.9	16.0	83.1
W. Gabriel, London	362/1000	Double overcoil, d r., fusee	−0.6	0 5	0.05	+0.8	+2.1	+1.9	+2 4	6.75	30.2	36.2	16.7	83.1
A. E. Fridlander, Coventry	52484†	Double overcoil, d r, g b	+3 9	0.4	0.07	+2.3	−0.6	−0.9	−2.8	5.0	31 4	36.3	15.2	82.9
H. Golay, London	147108	Double overcoil, d.r. g.b	+3.2	0.5	0 02	−3.8	+2 6	−0.1	+2.4	9 0	30.8	33.5	18.5	82.8
W. Holland, Rock Ferry	3614†	Single overcoil, d.r., g.b	+1.3	0.5	0.04	+1.6	−0 6	−3.3	−1.0	6 0	30.6	34.3	17.3	82.7
Baume & Co., London	2639	Single overcoil, d.r., g.b	+1.7	0.4	0.03	−0.7	−6.1	−2.3	−3.2	7.75	32.7	32.2	17.8	82.7
W. Holland, Rock Ferry	3578†	Single overcoil, d.r., fusee	+1 6	0 5	0.03	−2 4	+1.5	+2.9	+1.9	7.5	30.5	33.9	18.0	82.4
Usher & Cole, London	25768	Single overcoil, s r., g b.	+2.4	0.5	0.03	−2.9	+0 4	+1.1	+1 3	8.25	29.4	34.7	18.0	82.1

* d r., double-roller; s.r. single-roller, g b., going barrel. † Especially good.

Table I—*continued.*

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate. + Gaining. − Losing.	Mean variation of daily rate. ±	Mean change of rate for 1° F.	Difference of mean daily rate: Between pendant up and dial up.	Difference of mean daily rate: Between pendant up and pendant right.	Difference of mean daily rate: Between pendant up and pendant left.	Difference of mean daily rate: Between dial up and dial down.	Difference between extreme gaining and losing rates.	Marks awarded for: Daily variation of rate.	Marks awarded for: Change of rate with change of position.	Marks awarded for: Temperature compensation	Total Marks. 0—100.
			secs.	secs.	secs	secs	secs	secs.	secs.	secs.				
Baume & Co., London	3000†	Single overcoil, d.r., g.b., bar-lever	+2·5	0·5	0·03	−1·4	−1·7	+3·2	+1·5	8·75	29·4	34·8	17·8	82·0
H. Golay, London	14782	Double overcoil, d.r., g b.	+0·1	0·5	0·07	−2·6	−2·9	−1·9	+0·2	7·25	30·1	36·8	15·1	82·0
A. E. Fridlander, Coventry	52568†	Double overcoil, d.r., g b.	+1·4	0·5	0·05	+1·6	−0·8	+2·3	−1·6	4·75	29·7	35·7	16·5	81·9
Stauffer & Co., London	122290	Single overcoil, d.r., g.b.	+1·9	0·5	0·05	+1·0	−1·2	−3·4	+0·1	7·25	30·8	34·2	16·8	81·8
Baume & Co., London	3002†	Single overcoil, d r., g.b., bar-lever	+1·7	0·4	0·07	−1·0	−4·6	−4·2	−2·8	6·0	32·9	33·2	15·6	81·7
H. Golay, London	14780	Double overcoil, d.r., g b.	+1·4	0·6	0·002	−0·7	+0·9	−0·2	+5·3	9·0	27·7	34·1	19·9	81·7
Baume & Co., London	2702	Single overcoil, d.r., g b.	+1·3	0·7	0·03	+1·7	−0·7	−0·0	−0·9	5·75	26·5	37·1	18·0	81·6
G. Barter, London	14727	Double overcoil, d.r., g b.	+2·3	0·5	0·06	−0·7	−2·4	−0·9	+2·2	7·0	29·7	36·0	15·8	81·5
D. Keys, London	15839	Single overcoil, d.r., g.b.	+0·2	0·6	0·04	+1·0	+2·2	−0·5	+2·4	6·0	28·8	35·0	17·6	81·4
Usher & Cole, London	25811†	Single overcoil, s.r., g.b	−0·2	0·4	0·05	+0·7	+4·1	−2·9	+0·2	8·5	31·3	33·6	16·4	81·3

† Especially good.

Highest Records obtained by Complicated Watches during the year.

Description of watch.	Number.	Deposited by	Marks awarded for			Total marks, 0—100.
			Variation.	Position.	Temperature.	
Minute and seconds chronograph and repeater ..	52568	A. E. Fridlander, Coventry ...	29·7	35·7	16·5	81·9*
" " " " ..	14799	H. Golay, London	26·0	30·2	18·5	74·7
" " " " ..	14793	H. Golay, "	23·5	34·2	16·4	74·1
Split-seconds and minute-recorder chronograph .	2773	Baume and Co., London......	27·0	33·2	15·7	75·9
" " " " .	1070	H. Golay, "	24·0	31·3	16·9	72·2
" " " " .	2500	Baume and Co. "	27·8	31·8	12·5	72·1
Minute and seconds chronograph...........	52484	A. E. Fridlander, Coventry ...	31·4	36·3	15·2	82·9*
" " "	14780	H. Golay, London	27·7	34·1	19·9	81·7
" " "	79352	The English Watch Company, Birmingham	25·1	35·0	18·5	78·6
Perpetual calendar and repeater..	14782	H. Golay, London	30·1	36·8	15·1	82·0
" " "	14794	H. Golay, "	23·9	31·6	19·5	75·0
" " "	14792	H. Golay, "	28·6	32·9	11·2	72·7
Repeater	14727	G. Barter, London...........	29·7	36·0	15·8	81·5
"	14785	H. Golay, "	30·3	35·9	14·0	80·2*
"	14784	H. Golay, "	31·3	34·2	13·3	78·8*
Ordinary seconds chronograph	47147	Carley and Co., London.......	28·1	35·5	17·6	81·2
" " "	03603	E. F. Ashley, London........	29·5	34·6	14·7	78·8
" " "	80543	Rotherham and Sons, Coventry	24·9	29·2	18·3	72·4

* Especially good.

APPENDIX IV.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Portable Transit Instrument	1869
The Science and Art Department, South Kensington.	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo-Heliograph, Pendulum Apparatus, Dip-Circle, Unifilar, and Hodgkinson's Actinometer.	1876
Lieutenant A. Gordon, R.N.	Unifilar Magnetometer by Jones, No. 102, complete, with three Magnets and Deflection Bar. Dip-Circle, by Barrow, one Pair of Needles, and Magnetizing Bars. One Bifilar Magnetometer. One Declinometer. Two Tripod Stands.	1883
General Sir H. Lefroy, R.A., F.R.S.	Toronto Daily Registers for 1850–3	1885
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete. Pair 9-inch Dip-Needles with Bar Magnets	1883 1887
Professor O. J. Lodge	Unifilar Magnetometer, by Jones, No. 106, complete. Barrow Dip-Circle, No. 23, with two Needles, and Magnetizing Bars. Tripod Stand.	1883
Mr. W. F. Harrison.	Condensing lens and copper lamp chimney	1883
Captain W. de W. Abney, F.R.S.	Mason's Hygrometer, by Jones	1885
Professor Rücker	Tripod Stand	1886
Lord Rayleigh	Standard Barometer (Adie, No. 655)	1885
Mr. J. E. Cullum	Alt-Azimuth by Robinson, C. 42	1889

December 6, 1888.

Professor G. G. STOKES, D.C.L., President, followed by Dr. W. POLE, Vice-President, in the Chair.

The President announced that he had appointed as Vice-Presidents—

The Treasurer,
Sir James Paget,
Dr. Pole,
Sir Henry Roscoe.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Description of the Skull of an extinct Carnivorous Marsupial of the size of a Leopard (*Thylacopardus australis*, Ow.), from a recently opened Cave near the 'Wellington Cave' locality, New South Wales." By Sir RICHARD OWEN, K.C.B., F.R.S., &c. Received October 12, 1888.

[Publication deferred.]

II. "The Pectoral Group of Muscles." By BERTRAM C. A. WINDLE, M.A., M.D. (Dub.), Professor of Anatomy in the Queen's College, Birmingham. Communicated by Professor A. MACALISTER, F.R.S. Received October 25, 1888.

(Abstract.)

This paper is an attempt to explain the morphology of the pectoral group of muscles, and is based on the dissection of over fifty mammals, and on descriptions of others in various journals, &c.

The following are the chief conclusions:—

1. That portion of the lateral sheet of muscle, pushed outwards in the form of a cone by the growth of the anterior limb-bud, which belongs to the ventral region, may be divided by radial lines of fission into three segments, viz., an anterior or manubrial, a mesial or gladiolar, and a posterior or abdominal.

2. That the radial division is of primary importance is shown by the fact that each of these segments has its own nerve, viz., the anterior, a nerve corresponding to external anterior thoracic of human anatomy; the middle, internal anterior thoracic; the posterior, lateral thoracic. The first is definite in its origin and distribution, and the third in its origin, the second is less regular, and in correspondence with this is a certain indefiniteness of the line of division between the second and third segments.

3. The anterior segment may be subdivided into clavicular or manubrial portions, and the posterior may also be in two divisions, but these are not regarded as of primary value.

4. Each segment may undergo a secondary lamination into superficial and deep parts, viz., anterior into superficial and deep manubrial, middle into gladiolar and costal, and posterior into superficial and deep abdominal.

5. *Superficial manubrial* is always present and generally covers the others at its expanded insertion; it may be distinct or fused with deep manubrial or gladiolar, or both.

6. *Deep manubrial* may be absent, or present and distinct, or fused with, or just separable from, superficial. It may be fused with costal or very rarely with gladiolar, if the plane of manubrial lamination is more superficial than usual. The relation of this muscle to the so-called "sterno-scapularis" is discussed, the author being of opinion that the latter is subclavian in its nature.

7. *Gladiolar* may be absent or nearly so. It may be distinct or fused with superficial or deep manubrial, or with costal or abdominal. It is very often fused at its posterior border only with costal, the two sheets being otherwise separate.

8. *Costal* may arise from the edge of the sternum and the costal cartilages, from the cartilages alone, or from the ribs. It has a tendency as it decreases in size to shift its origin farther outwards, and its insertion farther towards the shoulder. It may be fused with gladiolar or deep manubrial or abdominal. It may consist of two portions, anterior and posterior.

9. *Abdominal* may be absent or double, and the two parts may overlie one another, or one may be anterior to the other. It may be fused with gladiolar or costal. It may be connected by its entire outer border with the dorsal sheet, thus closing the axilla, or fasciculi may pass from one side to the other (*achselbogen*). The origin may wander outwards to the lower ribs (*pectoralis quartus*).

10. The parts above described are very variously arranged amongst mammalia. The conditions obtaining are discussed and exhibited in a tabular form.

11. The various factors are thus represented in man:—

Superficial manubrial: clavicular and anterior part of *pectoralis major*, sometimes separate from the remainder of the muscle.

Deep manubrial: occasionally present as the *pectoralis minimus* of Wenzel Gruber.

Gladiolar: posterior, non-reflected part of *pectoralis major.*

Costal: double (1) *pectoralis minor,* (2) deep reflected part of *pectoralis major.*

Abdominal: occasionally present as *pectoralis quartus,* or as some of the forms of *achselbogen.*

III. "Some Observations on the Amount of Light reflected and transmitted by certain kinds of Glass." By Sir JOHN CONROY, Bart., M.A., Bedford Lecturer of Balliol College and Millard Lecturer of Trinity College, Oxford. Communicated by A. G. VERNON HARCOURT, Esq., F.R.S. Received November 8, 1888.

(Abstract.)

The experiments were commenced in order to determine the amount of light lost by transmission through glass.

Plates of the same kind of glass, but of different thickness, were taken, and the amount of light they transmitted determined, and from these values the percentage amounts reflected and obstructed calculated.

The amount reflected from the first surface was also determined directly by measuring the relative intensities of the illumination produced by two argand flames, when the light from both fell directly on the photometric surfaces, and when the light from one fell directly whilst that from the other reached the photometer after reflection from the surface of the glass.

Experiments were also made to ascertain whether repolishing altered in any way the reflective power of the glass; and the polarising angles of the glass before and after repolishing were also determined.

Conclusions.

It seems probable that the amount of light reflected by freshly polished glass varies with the way in which it has been polished, and that, if a perfect surface could be obtained without altering the refractive index of the surface-layer, then the amount would be accurately given by Fresnel's formula, but that usually the amount differs from that given by the formula, being sometimes greater and sometimes less.

The formation of a film of lower refractive index on the glass would account for the defect in the reflected light; but to account for the excess, it seems necessary to assume that the polishing has increased the optical density of the surface-layer, and the changes produced in the amount of light transmitted and in the angle of polarisation support this view.

After being polished, the surface of flint glass seems to alter somewhat readily, the amount of the reflected light decreasing, and the amount of the transmitted increasing, whilst with crown glass the change, if any, proceeds very slowly.

There is no evidence to show to what particular cause these changes are due.

The values of the transmission coefficients for light of mean refrangibility for the two particular kinds of glass are given, and show that for 1 cm. the loss by obstruction amounts to 2·62 per cent. with the crown glass and 1·15 per cent. with the flint glass.

IV. "The Specific Resistance and other Properties of Sulphur." By JAMES MONCKMAN, D.Sc. Communicated by Professor J. J. THOMSON, F.R.S. Received November 10, 1888.

[Publication deferred.]

Presents, December 6, 1888.

Transactions.

Freiburg-im-Breisgau:—Naturforschende Gesellschaft. Berichte. Bd. II. 8vo. *Freiburg* 1887. The Society.

Gloucester:—Cotteswold Naturalists' Field Club. Proceedings. 1887–1888. 8vo. *Gloucester*; The Origin of the Cotteswold Field Club. By W. C. Lucy. 8vo. *Gloucester* 1888. The Club.

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Halle:—Verein für Erdkunde. Mittheilungen. 1881. 8vo. *Halle.* 1881. The Verein.

Hamburg:—Naturhistorisches Museum. Bericht. 1887. 8vo. *Hamburg* 1888. The Museum.

Helsingfors:—Finska Vetenskaps-Societet. Acta. Tom. XV. 4to. *Helsingfors* 1888; Bidrag till Kännedom af Finlands Natur och Folk. Häftet 45–47. 8vo. *Helsingfors* 1887–88; Öfversigt. XXVIII–XXIX. 8vo. *Helsingfors* 1886–87; Des

Transactions (*continued*).

Organisation och Verksamhet. 1838–1888. 8vo. *Helsingfors.* The Society.

Hertfordshire Natural History Society. Transactions. Vol. IV. Parts 8–9. Vol. V. Part I. 8vo. *London* 1888. The Society.

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K. Akademie der Wissenschaften. Denkschriften (Math.-Naturw. Classe) Bd. LIII. 4to. *Wien* 1887; Denkschriften (Philos.-Histor. Classe). Bd. XXXVI. 4to. *Wien* 1888; Sitzungsberichte (Math.-Naturw. Classe). Abth. 1. Jahrg. 1887. Hefte 1–10. 8vo. *Wien* 1887–88. Abth. 2. Jahrg 1887. Hefte 3–10. 8vo. *Wien* 1887-88. Abth. 3. Jahrg 1887. Hefte 1–10. 8vo. *Wien* 1887–88; Sitzungsberichte (Philos.-Histor. Classe). Jahrg. 1887. Hefte 2–3. 8vo. *Wien* 1887–88; Almanach, 1887. 8vo. *Wien.* The Academy.

K. K. Geologische Reichsanstalt. Jahrbuch. Bd. XXXVII. Hefte 3–4. Bd. XXXVIII. Heft 3. 8vo. *Wien* 1888. The Institute.

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Basset (A. B.) A Treatise on Hydrodynamics. Vol. II. 8vo. *Cambridge* 1888. The Author.

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Gordon (C. A.) The Vivisection Controversy in Parliament. 8vo. *London* 1888. The Author.

Jones (T. R.), F.R.S., and H. Woodward, F.R.S. An Undescribed Carboniferous Fossil. 8vo. *London* 1888. With two other Excerpts in 8vo. Prof. Jones, F.R.S.

King (G.), F.R.S. The Species of Ficus of the Indo-Malayan and Chinese Countries. Part 2. Folio. *Calcutta* 1888. The Author.

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December 13, 1888.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Spectrum Analysis of Cadmium." By A. GRUNWALD, Professor of Mathematics in the Imp. Roy. German Polytechnic University at Prague. Communicated by Professor LIVEING, F.R.S. Received November 26, 1888.

[Publication deferred.]

II. "On the Bending and Vibration of thin elastic Shells, especially of Cylindrical Form." By LORD RAYLEIGH, M.A., D.C.L., Sec. R.S. Received December 1, 1888.

In a former publication* "On the Infinitesimal Bending of Surfaces of Revolution," I have applied the theory of bending to explain the deformation and vibration of thin elastic shells, which are symmetrical about an axis, and have worked out in detail the case where the shell is a portion of a sphere. The validity of this application depends entirely upon the principle that when the shell is thin enough and is vibrating in one of the graver possible modes, the middle surface behaves as if it were inextensible. "When a thin sheet of matter is subjected to stress, the force which it opposes to extension is great in comparison with that which it opposes to bending. Under ordinary circumstances, the deformation takes place approximately as if the sheet were inextensible as a whole, a condition which, in a remarkable degree, facilitates calculation, though (it need scarcely be said) even bending implies an extension of all but the central layers." If we fix our attention upon one of the terms involving sines or cosines of multiples of the longitude, into which, according to Fourier's theorem, the whole deformation may be resolved, the condition of inextensibility is almost enough to define the type. If there are two edges, *e.g.*, parallel to circles of lati-

* 'London Math. Soc. Proc.,' vol. 13, p. 4, November, 1881.

tude, the solution contains two arbitrary constants; but if a pole be included, as when the shell is in the form of a hemisphere, one of the constants vanishes, and the type of deformation is wholly determined, without regard to any other mechanical condition, to be satisfied at the edge or elsewhere. It will be convenient to restate, analytically, the type of deformation arrived at [equation (5)]. If the point upon the middle surface, whose coordinates were originally a, θ, ϕ, moves to $a+\delta r$, $\theta+\delta\theta$, $\phi+\delta\phi$, the solution is

$$\left.\begin{aligned} \delta\phi &= \mathrm{A}\tan^s\tfrac{1}{2}\theta\cos s\phi \\ \delta\theta &= -\mathrm{A}\sin\theta\tan^s\tfrac{1}{2}\theta\sin s\phi \\ \delta r &= \mathrm{A}a\,(s+\cos\theta)\tan^s\tfrac{1}{2}\theta\sin s\phi \end{aligned}\right\}\ldots\ldots\ldots\ldots(1),$$

θ being the colatitude measured from the pole through which the shell is complete. Any integral value higher than unity is admissible for s. The value 0 and 1 correspond to displacements not involving strain.

In a recent paper* Mr. Love dissents from the general principle involved in the theory above briefly sketched, and rejects the special solutions founded upon it as inapplicable to the vibration of thin shells. The argument upon which I proceeded in my former paper, and which still seems to me valid, may be put thus: It is a general mechanical principle† that, if given displacements (not sufficient by themselves to determine the configuration) be produced in a system originally in equilibrium by forces of corresponding types, the resulting deformation is determined by the condition that the potential energy of deformation shall be as small as possible. Apply this to an elastic shell, the given displacements being such as not of themselves to involve a stretching of the middle surface.‡ The resulting deformation will, in general, include both stretching and bending, and any expression for the energy will contain corresponding terms proportional to the first and third powers respectively of the thickness. This energy is to be as small as possible. Hence, when the thickness is diminished without limit, the actual displacement will be one of pure bending, if such there be, consistent with the given conditions. Otherwise the energy would be of the first order (in thickness) instead of, as it might be, of the third order, in violation of the principle.

It will be seen that this argument takes no account of special conditions to be satisfied at the edge of the shell. This is the point at which Mr. Love concentrates his objections. He considers that

* "On the small free Vibrations and Deformation of a thin elastic Shell," 'Phil. Trans.,' A, 1888.

† 'Phil. Mag.,' March, 1875; 'Theory of Sound,' § 74.

‡ There are cases where no displacement (involving strain at all) is possible without stretching of the middle surface, *e.g.*, that of the complete sphere.

the general condition necessary to be satisfied at a free edge is in fact violated by such a deformation as (1). But the condition in question* contains terms proportional to the first and to the third powers respectively of the thickness, the coefficients of the former involving as factors the extensions and shear of the middle surface. It appears to me that when the thickness is diminished without limit, the fulfilment of the boundary condition requires only that the middle surface be unstretched, precisely the requirement satisfied by solutions such as (1).

Of course, so long as the thickness is finite, the forces in operation will entail some stretching of the middle surface, and the amount of this stretching will depend on circumstances. A good example is afforded by a circular cylinder with plane edges perpendicular to the axis. Let normal forces locally applied at the extremities of one diameter of the central section cause a given shortening of that diameter. That the potential energy may be a minimum, the deformation must assume more and more the character of mere bending as the thickness is reduced. The only kind of bending that can occur in this case is the purely cylindrical one in which every normal section is similarly deformed, and then the potential energy is proportional to the total length of the cylinder. We see, therefore, that if the cylinder be very long, the energy of bending corresponding to the given local contraction of the central diameter may become very great, and a heavy strain is thrown upon the principle that the deformation of minimum energy is one of pure bending.

If the small thickness of the shell be regarded as given, a point will at last be attained when the energy can be made least by a sensible local stretching of the middle surface such as will dispense with the uniform bending otherwise necessary over so great a length. But even in this extreme case it seems correct to say that, when the thickness is *sufficiently* reduced, the deformation tends to become one of pure bending.

At first sight it may appear strange that of two terms in an expression of the potential energy, the one proportional to the cube of the thickness is to be retained, while that proportional to the first power may be omitted. The fact, however, is that the large potential energy which would accompany any stretching of the middle surface is the very reason why such stretching will not occur. The comparative largeness of the coefficient (proportional to the first power of the thickness) is more than neutralised by the smallness of the stretching itself, to the *square* of which the energy is proportional.

In general, if ψ_1 be the coordinate measuring the violation of the tie which is supposed to be more and more insisted upon by increasing

* See his equation (33).

stiffness, and if the other coordinates be suitably chosen, the potential energy of the system may be expressed

$$V = \tfrac{1}{2}c_1\psi_1^2 + \tfrac{1}{2}c_2\psi_2^2 + \tfrac{1}{2}c_3\psi_3^2 +$$

This follows from the general theorem that V and T may always be reduced to sums of squares simply, if we suppose that $T = \tfrac{1}{2}a_1\dot{\psi}_1^2$.

The equations of equilibrium under the action of external forces $\Psi_1, \Psi_2, \ldots$ are thus

$$\Psi_1 = c_1\psi_1, \qquad \Psi_2 = c_2\psi_2, \ldots\ldots;$$

hence if the forces are regarded as given, the effect of increasing c_1 without limit is not merely to annul ψ_1, but also the term in V which depends upon it.

An example might be taken from the case of a rod clamped at one end A, and deflected by a lateral force, whose stiffness from the end A up to a neighbouring place B, is conceived to increase indefinitely. In the limit we may regard the rod as clamped at B, and neglect the energy of the part AB, in spite of, or rather in consequence of, its infinite stiffness.

If it be admitted that the deformations to be considered are pure bendings, the next step is the calculation of the potential energy corresponding thereto. In my former paper, the only case for which this part of the problem was attempted was that of the sphere. After bending, "the principal curvatures differ from the original curvature of the sphere in opposite directions, and to an equal amount,* and the potential energy of bending corresponding to any element of the surface is proportional to the square of this excess or defect of curvature, without regard to the direction of the principal planes." Though he agrees with my conclusions, Mr. Love appears to regard the argument as insufficient. But clearly in the case of a given spherical shell, there are no other elements upon which the energy of bending could depend. "Thus the energy corresponding to the element of surface $a^2\sin\theta\,d\theta\,d\phi$ may be denoted by

$$a^2 H(\delta\rho^{-1})^2 \sin\theta\,d\theta\,d\phi \quad \ldots\ldots\ldots\ldots\ldots\ldots (2),$$

where H depends upon the material and upon the thickness."

By the nature of the case H is proportional to the elastic constants and to the cube of the thickness, from which it follows by the method of dimensions that it is independent of a, the radius of the sphere.

* This is in virtue of Gauss's theorem that the product of the principal curvatures is unaffected by bending.

I did not, at the time, attempt the further determination of H, not needing it for my immediate purpose. Mr. Love has shown that

$$H = \tfrac{4}{3} n h^3 \quad \dots\dots\dots\dots\dots\dots\dots (3),$$

where $2h$ represents the thickness, and n is the constant of rigidity. Why n alone should occur, to the exclusion of the constant of compressibility, will presently appear more clearly.

The application of (2) to the displacements expressed in (1) gave [equation (18)]

$$V = 2\pi \Sigma\, (s^3 - s)\, A_s^2 \int_0^\theta H \sin^{-3}\theta \tan^{2s}\tfrac{1}{2}\theta \, d\theta \quad \dots\dots\dots (4),$$

θ being the colatitude of the (circular) edge. In the case of the hemisphere of uniform thickness

$$V = \tfrac{1}{2}\pi H \Sigma\, (s^3 - s)(2s^2 - 1)\, A_s^2 \quad \dots\dots\dots\dots\dots (5).$$

The calculation of the pitch of free vibration then presented no difficulty. If σ denote the superficial density, and $\cos pt$ represent the type of vibration, p_2 corresponding to $s = 2$, p_3 to $s = 3$, and so on, it appeared that

$$p_2 = \frac{\sqrt{H}}{a^2\sigma} \times 5{\cdot}2400, \qquad p_3 = \frac{\sqrt{H}}{a^2\sigma} \times 14{\cdot}726, \qquad p_4 = \frac{\sqrt{H}}{a^2\sigma} \times 28{\cdot}462\,;$$

so that

$$p_3/p_2 = 2{\cdot}8102, \qquad\qquad p_4/p_2 = 5{\cdot}4316,$$

determining the *intervals* between the graver notes.

If the form of the shell be other than spherical, the middle surface is no longer symmetrical with respect to the normal at any point, and the expression of the potential energy is more complicated. The question is now not merely one of the curvature of the deformed surface; account must also be taken of the correspondence of normal sections before and after deformation.* A complete investigation has been given by Love; but the treatment of the question now to be explained, even if less rigorous, may help to throw light upon this somewhat difficult subject.

In the actual deformation of a material sheet of finite extent there will usually be at any point not merely a displacement of the point itself, but a rotation of the neighbouring parts of the sheet, such as a

* An extreme case may serve as an illustration. Suppose that the bending is such that the principal planes retain their positions relatively to the material surface, but that the principal curvatures are exchanged. The nature of the curvature at the point in question is the same after deformation as before, and by a rotation through 90° round the normal the surfaces may be made to fit; nevertheless the energy of bending is finite.

rigid body may undergo. All this contributes nothing to the energy. In order to take the question in its simplest form, let us refer the original surface to the normal and principal tangents at the point in question as axes of coordinates, and let us suppose that after deformation, the lines in the sheet originally coincident with the principal tangents are brought back (if necessary) to occupy the same positions as at first. The possibility of this will be apparent when it is remembered that in virtue of the inextensibility of the sheet, the angles of intersection of all lines traced upon it remain unaltered. The equation of the original surface in the neighbourhood of the point being

$$z = \tfrac{1}{2}\left(\frac{x^2}{\rho_1} + \frac{y^2}{\rho_2}\right) \quad \ldots\ldots\ldots\ldots\ldots\ldots \text{(6)},$$

that of the deformed surface may be written

$$z = \tfrac{1}{2}\left\{\frac{x^2}{\rho_1+\delta\rho_1} + \frac{y^2}{\rho_2+\delta\rho_2} + 2\tau xy\right\} \quad \ldots\ldots\ldots\ldots \text{(7)}.$$

In strictness $(\rho_1 + \delta\rho_1)^{-1}$, $(\rho_2 + \delta\rho_2)^{-1}$ are the curvatures of the sections made by the planes $x = 0$, $y = 0$; but since principal curvatures are a maximum or a minimum, they represent with sufficient accuracy the new principal curvatures, although these are to be found in slightly different planes. The condition of inextensibility shows that points which have the same x and y in (6) and (7) are *corresponding* points, and by Gauss's theorem it is further necessary that

$$\frac{\delta\rho_1}{\rho_1} + \frac{\delta\rho_2}{\rho_2} = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(8)}.$$

It thus appears that the energy of bending will depend upon two quantities, one giving the alterations of principal curvature, and the other τ depending upon the shift (in the material) of the principal planes.

In calculating the energy we may regard it as due to the stretchings and contractions under tangential forces of the various infinitely thin laminæ into which the shell may be divided. The middle lamina being unstretched, makes no contribution. Of the other laminæ, the stretching is in proportion to the distance from the middle surface, and the energy of stretching is therefore as the square of this distance. When the integration over the whole thickness of the shell is carried out, the result is accordingly proportional to the cube of the thickness.

The next step is to estimate more precisely the energy corresponding to a small element of area of a lamina. The general equations in

three dimensions, as given in Thomson and Tait's 'Natural Philosophy,' § 694, are

$$na = \mathrm{S}, \qquad nb = \mathrm{T}, \qquad nc = \mathrm{U} \quad \ldots\ldots\ldots (9)$$

$$\left.\begin{aligned} \mathrm{M}e &= \mathrm{P}-\sigma\,(\mathrm{Q}+\mathrm{R}) \\ \mathrm{M}f &= \mathrm{Q}-\sigma\,(\mathrm{R}+\mathrm{P}) \\ \mathrm{M}g &= \mathrm{R}-\sigma\,(\mathrm{P}+\mathrm{Q}) \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots (10),$$

where
$$\rho = \frac{m-n}{2m} \quad \ldots\ldots\ldots\ldots\ldots\ldots (11).*$$

The energy w, corresponding to the unit of volume, is given by

$$2w = (m+n)\,(e^2+f^2+g^2) \\ +2\,(m-n)\,(fg+ge+ef)+n(a^2+b^2+c^2) \quad \ldots\ldots (12).$$

In the application to a lamina, supposed parallel to xy, we are to take $\mathrm{R} = 0$, $\mathrm{S} = 0$, $\mathrm{T} = 0$; so that

$$g = -\sigma\,\frac{e+f}{1-\sigma}, \qquad a = 0, \qquad b = 0.$$

Thus in terms of the elongations e, f, parallel to x, y, and of the shear c, we get

$$w = n\left\{e^2+f^2+\frac{m-n}{m+n}\,(e+f)^2+\tfrac{1}{2}\,c^2\right\} \quad \ldots\ldots\ldots (13).$$

We have now to express the elongations of the various laminæ of a shell when bent, and we will begin with the case where $\tau = 0$, that is, when the principal planes of curvature remain unchanged. It is evident that in this case the shear c vanishes, and we have to deal only with the elongations e and f parallel to the axes. In the section by the plane of zx, let s, s' denote corresponding infinitely small arcs of the middle surface and of a lamina distant h from it. If ψ be the angle between the terminal normals, $s = \rho_1\psi$, $s' = (\rho_1+h)\,\psi$, $s'-s = h\,\psi$. In the bending, which leaves s unchanged,

$$\delta s' = h\,\delta\psi = hs\,\delta\,(1/\rho_1).$$

Hence

$$e = \delta s'/s' = h\,\delta(1/\rho_1),$$

and in like manner $f = h\,\delta\,(1/\rho_2)$. Thus for the energy U per unit of area we have

* M is Young's modulus, σ is Poisson's ratio, n is the constant of rigidity, and $(m-\frac{1}{3}n)$ that of cubic compressibility. In terms of Lamé's constants (λ, μ), $m = \lambda+\mu$, $n = \mu$.

$$dU = nh^2 dh \left\{ \left(\delta\frac{1}{\rho_1}\right)^2 + \left(\delta\frac{1}{\rho_2}\right)^2 + \frac{m-n}{m+n}\left(\delta\frac{1}{\rho_1} + \delta\frac{1}{\rho_2}\right)^2 \right\},$$

and on integration over the whole thickness of the shell ($2h$) *

$$U = \frac{2nh^3}{3}\left\{ \left(\delta\frac{1}{\rho_1}\right)^2 + \left(\delta\frac{1}{\rho_2}\right)^2 + \frac{m-n}{m+n}\left(\delta\frac{1}{\rho_1} + \delta\frac{1}{\rho_2}\right)^2 \right\} \ldots (14).$$

This conclusion may be applied at once, so as to give the result applicable to a spherical shell; for, since the original principal planes are arbitrary, they can be taken so as to coincide with the principal planes after bending. Thus $\tau = 0$; and by Gauss's theorem

$$\delta\frac{1}{\rho_1} + \delta\frac{1}{\rho_2} = 0,$$

so that

$$U = \frac{4nh^3}{3}\left(\delta\frac{1}{\rho_1}\right)^2 \ldots\ldots\ldots\ldots\ldots (15),$$

where $\delta\rho^{-1}$ denotes the change of principal curvature. Since $e = -f$, $g = 0$, the various laminæ are simply sheared, and that in proportion to their distance from the middle surface. The energy is thus a function of the constant of rigidity only.

The result (14) is applicable directly to the plane plate; but this case is peculiar in that, on account of the infinitude of ρ_1, ρ_2 (8) is satisfied without any relation between $\delta\rho_1$ and $\delta\rho_2$. Thus for a plane plate

$$U = \frac{2nh^3}{3}\left\{ \frac{1}{\rho_1^2} + \frac{1}{\rho_2^2} + \frac{m-n}{m+n}\left(\frac{1}{\rho_1} + \frac{1}{\rho_2}\right)^2 \right\} \ldots\ldots (16),$$

where ρ_1^{-1}, ρ_2^{-1}, are the two independent principal curvatures after bending.

We have thus far considered τ to vanish; and it remains to investigate the effect of the deformations expressed by

$$\delta z = \tau xy = \tfrac{1}{2}\tau(\xi^2 - \eta^2) \ldots\ldots\ldots\ldots\ldots (17),$$

where ξ, η relate to new axes inclined at 45° to those of x, y. The curvatures defined by (17) are in the planes of ξ, η, equal in numerical value and opposite in sign. The elongations in these directions for

* It is here assumed that m and n are independent of h, that is, that the material is homogeneous. If we discard this restriction, we may form the conception of a shell of given thickness, whose middle surface is physically inextensible, while yet the resistance to bending is moderate. In this way we may realise the types of deformation discussed in the present paper, *without supposing the thickness to be infinitely small*; and the independence of such types upon conditions to be satisfied at a free edge is perhaps rendered more apparent.

any lamina within the thickness of the shell are $h\tau$, $-h\tau$, and the corresponding energy (as in the case of the sphere just considered) takes the form

$$U' = \frac{4nh^3\tau^2}{3} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (18).$$

This energy is to be added* to that already found in (14); and we get finally

$$U = \frac{2nh^3}{3}\left\{\left(\delta\frac{1}{\rho_1}\right)^2 + \delta\left(\frac{1}{\rho_2}\right)^2 + \frac{m-n}{m+n}\left(\delta\frac{1}{\rho_1}+\delta\frac{1}{\rho_2}\right)^2 + 2\tau^2\right\} \ldots (19),$$

as the complete expression of the energy, when the deformation is such that the middle surface is unextended. We may interpret τ by means of the angle χ, through which the principal planes are shifted; thus

$$\tau = 2\chi\left(\frac{1}{\rho_2} - \frac{1}{\rho_1}\right) \ldots\ldots\ldots\ldots\ldots\ldots\ldots (20).$$

It will now be in our power to treat more completely a problem of great interest, viz., the deformation and vibration of a cylindrical shell. In my former paper I investigated the types of bending, but without a calculation of the corresponding energy. The results were as follows.† If the cylinder be referred to columnar coordinates z, r, ϕ, so that the displacements of a point whose equilibrium coordinates are z, a, ϕ are denoted by δz, δr, $a\delta\phi$, the equations expressing inextensibility take the form

$$\frac{d\delta z}{dz} = 0, \qquad \delta r + a\frac{d\delta\phi}{d\phi} = 0, \qquad \frac{d\delta z}{d\phi} + a^2\frac{d\delta\phi}{dz} = 0 \ldots\ldots (21),$$

from which we may deduce

$$\frac{d^2\delta\phi}{dz^2} = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (22).$$

By (22), if $\delta\phi \propto \cos s\phi$, we may take

$$a\,\delta\phi = (A_s a + B_s z)\cos s\phi \ldots\ldots\ldots\ldots\ldots\ldots (23),$$

and then, by (21)

$$\delta r = s\,(A_s a + B_s z)\sin s\phi \ldots\ldots\ldots\ldots\ldots (24),$$

$$\delta z = -s^{-1}B_s a \sin s\phi \ldots\ldots\ldots\ldots\ldots\ldots (25).$$

* There are clearly no terms involving the products of τ with the changes of principal curvature $\delta(\rho_1^{-1})$, $\delta(\rho_2^{-1})$; for a change in the sign of τ can have no influence upon the energy of the deformation defined by (7).

† The method of investigation is similar to that employed by Jellet in his memoir ("On the Properties of Inextensible Surfaces," 'Irish Acad. Trans.,' vol. 22, 1855, p. 179), to which reference should have been made.

If the cylinder be complete, s is integral; A_s and B_s are independent constants, either of which may vanish. In the latter case the displacement is in two dimensions only.* It is unnecessary to stop to consider the demonstrations of (21), inasmuch as these equations will present themselves independently in the course of the investigations which follows.

It will be convenient to replace δz, δr, $a\delta\phi$ by single letters, which, however, it is difficult to choose so as not to violate some of the usual conventions. In conformity with Mr. Love's general notation, I will write

$$\delta z = u, \qquad a\,\delta\phi = v, \qquad \delta r = w \; \ldots\ldots\ldots \; (26).$$

The problem before us is the expression of the changes of principal curvature and shifts of principal planes at any point P (z, ϕ) of the cylinder in terms of the displacements u, v, w. As in (6), take as fixed co-ordinate axes the principal tangents and normal to the undisturbed cylinder at the point P, the axis of x being parallel to that of the cylinder, that of y tangential to the circular section, and that of ζ normal, measured inwards. If, as it will be convenient to do, we measure z and ϕ from the point P, we may express the undisturbed coordinates of a material point Q in the neighbourhood of P, by

$$x = z, \qquad y = a\phi, \qquad \zeta = \tfrac{1}{2}a\phi^2 \; \ldots\ldots\ldots\ldots \; (27).$$

During the displacement the coordinates of Q will receive the increments

$$u, \quad w \sin\phi + v\cos\phi, \quad -w\cos\phi + v\sin\phi;$$

so that after displacement

$$x = z + u, \qquad y = a\phi + w\phi + v(1 - \tfrac{1}{2}\phi^2),$$

$$\zeta = \tfrac{1}{2}a\phi^2 - w(1 - \tfrac{1}{2}\phi^2) + v\phi;$$

or if u, v, w be expanded in powers of the small quantities z, ϕ,

$$x = z + u_0 + \frac{du}{dz_0}z + \frac{du}{d\phi_0}\phi + \; \ldots\ldots\ldots\ldots \; (28).$$

$$y = a\phi + w_0\phi + v_0 + \frac{dv}{dz_0}z + \frac{dv}{d\phi_0}\phi + \ldots\ldots\ldots\ldots \; (29).$$

* See 'Theory of Sound,' § 233.

$$\zeta = \tfrac{1}{2}a\phi^2 - w_0 - \frac{dw}{dz_0}z - \frac{dw}{d\phi_0}\phi + v_0\phi$$

$$+\tfrac{1}{2}w_0\phi^2 - \tfrac{1}{2}\frac{d^2w}{dz_0^2}z^2 - \frac{d^2w}{dz_0 d\phi_0}z\phi - \tfrac{1}{2}\frac{d^2w}{d\phi_0^2}\phi^2$$

$$+\frac{dv}{dz_0}z\phi + \frac{dv}{d\phi_0}\phi^2 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (30),$$

u_0, v_0, . . . being the values of u, v at the point P.

These equations give the coordinates of the various points of the deformed sheet. We have now to suppose the sheet moved as a rigid body so as to restore the position (as far as the first power of small quantities is concerned) of points infinitely near P. A purely translatory motion by which the displaced P is brought back to its original position will be expressed by the simple omission in (28), (29), (30) of the terms u_0, v_0, w_0 respectively, which are independent of z, ϕ. The effect of an arbitrary rotation is represented by the additions to x, y, ζ respectively of $y\theta_3 - \zeta\theta_2$, $\zeta\theta_1 - x\theta_3$, $x\theta_2 - y\theta_1$; where for the present purpose θ_1, θ_2, θ_3 are small quantities of the order of the deformation, the square of which is to be neglected throughout. If we make these additions to (28), &c., substituting for x, y, ζ in the terms containing θ their approximate values, we find so far as the first powers of z, ϕ

$$x = z + \frac{du}{dz_0}z + \frac{du}{d\phi_0}\phi + a\phi\theta_3,$$

$$y = a\phi + w_0\phi + \frac{dv}{dz_0}z + \frac{dv}{d\phi_0}\phi - z\theta_3,$$

$$\zeta = \frac{dw}{dz_0}z - \frac{dw}{d\phi_0}\phi + v_0\phi + z\theta_2 - a\phi\theta_1.$$

Now, since the sheet is assumed to be inextensible, it must be possible so to determine θ_1, θ_2, θ_3 that to this order $x = z$, $y = a\phi$, $\zeta = 0$.

Hence $$\frac{du}{dz_0} = 0, \qquad \frac{du}{d\phi_0} + a\theta_3 = 0,$$

$$\frac{du}{dz_0} - \theta_3 = 0, \qquad w_0 + \frac{dv}{d\phi_0} = 0,$$

$$-\frac{dw}{dz_0} + \theta_2 = 0, \qquad \frac{dw}{d\phi_0} - v_0 + a\theta_1 = 0.$$

The conditions of inextensibility are thus (if we drop the suffices as no longer required)

$$\frac{du}{dz} = 0, \qquad w + \frac{dv}{d\phi} = 0, \qquad \frac{du}{d\phi} + a\frac{dv}{dz} = 0 \quad \ldots \; (31),$$

which agree with (21).

Returning to (28), &c., as modified by the addition of the translatory and rotatory terms, we get

$$x = z + \text{terms of 2nd order in } z,\ \phi,$$

$$y = a\phi + \quad ,, \quad ,,$$

$$\zeta = \tfrac{1}{2}a\phi^2 + \tfrac{1}{2}w_0\phi^2 - \tfrac{1}{2}\frac{d^2w}{dz_0^2}z^2 - \frac{d^2w}{dz_0 d\phi_0}z\phi$$

$$-\tfrac{1}{2}\frac{d^2w}{d\phi_0^2}\phi^2 + \frac{dv}{dz_0}z\phi + \frac{dv}{d\phi_0}\phi^2;$$

or since by (31) $d^2w/dz^2 = 0$, and $dv/d\phi = -w$,

$$\zeta = \tfrac{1}{2}a\phi^2 - \tfrac{1}{2}w_0\phi^2 - \frac{d^2w}{dz_0 d\phi_0}z\phi - \tfrac{1}{2}\frac{d^2w}{d\phi_0^2}\phi^2 + \frac{dv}{dz_0}z\phi.$$

The equation of the deformed surface after transference is thus

$$\zeta = xy\left\{\frac{1}{a}\frac{dv}{dz_0} - \frac{1}{a}\frac{d^2w}{dz_0 d\phi_0}\right\} + y^2\left\{\frac{1}{2a} - \frac{1}{2a^2}w_0 - \frac{1}{2a^2}\frac{d^2w}{d\phi_0^2}\right\} \quad \ldots \; (32).$$

Comparing with (7) we see that

$$\frac{1}{\rho_1} = 0, \qquad \delta\frac{1}{\rho_2} = -\frac{1}{a^2}\left(w + \frac{d^2w}{d\phi^2}\right), \qquad \tau = \frac{1}{a}\left(\frac{dv}{dz} - \frac{d^2w}{dz\,d\phi}\right) \quad \ldots \; (33);$$

so that by (19)

$$U = \frac{4nh^3}{3a^2}\left\{\frac{m}{m+n}\frac{1}{a^2}\left(w + \frac{d^2w}{d\phi^2}\right)^2 + \left(\frac{dv}{dz} - \frac{d^2w}{dz\,d\phi}\right)^2\right\} \quad \ldots \; (34).$$

This is the potential energy of bending reckoned per unit of area. It can if desired be expressed by (31) entirely in terms of v.*

We will now apply (24) to calculate the whole potential energy of a complete cylinder, bounded by plane edges $z = \pm l$, and of thick-

* From Mr. Love's general equations (12), (13), (18) a concordant result may be obtained by introduction of the special conditions—

$$k_1 = 0, \qquad k_2 = 1/a, \qquad 1/\rho_1 = 0, \qquad 1/\rho_2 = 1/a,$$

limiting the problem to the case of the cylinder, and of those

$$\sigma_1 = \sigma_2 = \varpi = 0,$$

which express the inextensibility of the middle surface.

ness which, if variable at all, is a function of z only. Since u, v, w are periodic when ϕ increases by 2π, their most general expression in accordance with (31) is [compare (23), &c.]

$$v = \Sigma[(A_s a + B_s z)\cos s\phi - (A_s' a + B_s' z)\sin s\phi] \ldots\ldots \quad (35),$$

$$w = \Sigma[s\,(A_s a + B_s z)\sin s\phi + s\,(A_s' a + B_s' z)\cos s\phi] \ldots \quad (36),$$

$$u = \Sigma[-s^{-1}B_s a \sin s\phi - s^{-1}B_s' a \cos s\phi] \ldots\ldots\ldots\ldots \quad (37),$$

in which the summation extends to all integral values of s from 0 to ∞. But the displacements corresponding to $s = 0$, $s = 1$ are such as a rigid body might undergo, and involve no absorption of energy. When the values of u, v, w are substituted in (34) all the terms containing products of sines or cosines with different values of s vanish in the integration with respect to ϕ, as do also those which contain $\cos s\phi \sin s\phi$. Accordingly

$$\int_0^{2\pi} Ua\,d\phi = \frac{4\pi n h^3}{3a}\left[\frac{m}{m+n}\frac{1}{a^2}\Sigma\,(s^3-s)^2\right.$$

$$\left.\{(A_s a + B_s z)^2 + (A_s' a + B_s' z)^2\} + \Sigma\,(s^2-1)^2\,(B_s^2 + B_s'^2)\right] \ldots \quad (38).$$

Thus far we might consider h to be a function of z; but we will now treat it as a constant. In the integration with respect to z the odd powers of z will disappear, and we get as the energy of the whole cylinder of radius a, length $2l$, and thickness $2h$,

$$\int_{-l}^{+l}\int_0^{2\pi} Ua\,d\phi\,dz$$

$$= \frac{8\pi n h^3 l}{3a}\,\Sigma\,(s^2-1)^2\left[\frac{m\,.\,s^2}{m+n}\left\{A_s^2 + A_s'^2\right.\right.$$

$$\left.\left. + \frac{l^2}{3a^2}\,(B_s^2 + B_s'^2)\right\} + B_s^2 + B_s'^2\right] \ldots\ldots\ldots\ldots (39),$$

in which $s = 2, 3, 4, \ldots$.

The expression (39) for the potential energy suffices for the solution of statical problems. As an example we will suppose that the cylinder is compressed along a diameter by equal forces F, applied at the points $z = z_1$, $\phi = 0$, $\phi = \pi$, although it is true that so highly localised a force hardly comes within the scope of the investigation in consequence of the stretchings of the middle surface, which will

occur in the immediate neighbourhood of the points of application.*

The work done upon the cylinder by the forces F during the hypothetical displacement indicated by δA_s, &c., will be by (36)

$$-F\Sigma s\,(a\delta A_s'+z_1\delta B_s')\,(1+\cos s\pi),$$

so that the equations of equilibrium are

$$\frac{dv}{dA_s}=0, \qquad \frac{dv}{dB_s}=0.$$

$$\frac{dv}{dA_s'}=-(1+\cos s\pi)\,saF, \qquad \frac{dv}{dB_s'}=-(1+\cos s\pi)\,sz_1F.$$

Thus for all values of s,

$$A_s=B=0;$$

and for odd values of s,

$$A_s'=B_s'=0.$$

But when s is even,

$$\frac{ms^2}{m+n}A_s'=-\frac{3sa^2F}{8\pi nh^3l(s^2-1)^2} \quad\ldots\ldots\ldots\ldots\quad (40),$$

$$\left\{\frac{ms^2}{m+n}\,\frac{l^2}{3a^2}+1\right\}B_s'=-\frac{3saz_1F}{8\pi nh^3l(s^2-1)^2}\quad\ldots\ldots\ldots\ldots\quad (41);$$

and the displacement w at any point (z,ϕ) is given by

$$w=2\,(A_2'a+B_2'z)\cos 2\phi+4\,(A_4'a+B_4'z)\cos 4\phi+\ldots\ldots\ldots\ldots\quad (42),$$

where A_2', B_2', A_4', ... are determined by (40), (41).

If the cylinder be moderately long in proportion to its diameter, the second term in the left hand member of (41) may be neglected, so that

$$\frac{l^2}{3a^2}\,\frac{B_s'}{z_1}=\frac{A_s'}{a}.$$

In this case (42) may be written

$$w=\left(1+\frac{3z_1z}{l^2}\right)\{2A_2'a\cos 2\phi+4A_4'a\cos 4\phi+\ldots.\}\ldots\quad (43),$$

* Whatever the curvature of the surface, an area upon it may be taken so small as to behave like a plane, and therefore bend, in violation of Gauss's condition, when subjected to a force which is so nearly discontinuous that it varies sensibly within the area.

showing that, except as to magnitude and sign, the curve of deformation is the same for all values of z_1 and z.*

If $z = \pm z_1$, the amplitudes are in the ratio $1 \pm 3z_1^2/l^2$; and if, further, $z_1 = l$, *i.e.*, if the force be applied at one of the ends of the cylinder, the amplitudes are as $2 : -1$. The section where the deformation (as represented by w) is zero, is given by $3zz_1 + l^2 = 0$, in which if $z_1 = l$, $z = -\frac{1}{3}l$.

When the condition as to the length of the cylinder is not imposed, the ratio $B_s' : A_s'$ is dependent upon s, and therefore the curves of deformation vary with z, apart from mere magnitude and sign. If, however, we limit ourselves to the more important term $s = 2$, we have

$$\frac{4m}{m+n}\frac{A_2'}{a} = \left\{\frac{4m}{m+n}\frac{l^2}{3a^2} + 1\right\}\frac{B_2'}{z_1},$$

and $$w = 2B_2'\left\{\frac{a^2}{z_1}\left(\frac{l^2}{3a^2} + \frac{m+n}{4m}\right) + z\right\}\cos 2\phi;$$

so that w vanishes when

$$\frac{zz_1}{a^2} + \frac{l^2}{3a^2} + \frac{m+n}{4m} = 0 \quad\ldots\ldots\ldots\ldots\ldots\ldots (44).$$

This equation may be applied to find what is the length of the cylinder when the deformation just vanishes at one end if the force is applied at the other. If $z_1 = -z = l$,

$$\frac{l}{a} = \sqrt{\Big\{}$$

For many materials σ [equation (11)] is about $\frac{1}{4}$, or $m = 2n$. In such cases the condition is

$$l = \tfrac{3}{4}a.$$

It should not be overlooked that although w may vanish, u remains finite.

Reverting to (23), (24), (25) we see that, if the cylinder is open at both ends, there are two types of deformation possible for each value of s. If we suppose the cylinder to be closed at $z = 0$ by a flat disk attached to it round the circumference, the inextensibility of the disk imposes the conditions, $w = \delta r = 0$, $v = a\,\delta\phi = 0$, when $z = 0$.† Hence $A_s = 0$, and the only deformation now possible is

* That w is unaltered when z and z_1 are interchanged is an example of the general law of reciprocity.

† s being greater than 1.

$$\left.\begin{aligned} v &= a\,\delta\phi = \mathrm{B}_s z\cos s\phi \\ w &= \delta r \quad = s\mathrm{B}_s z \sin s\phi \end{aligned}\right\} \quad\ldots\ldots\ldots\ldots\ldots\ldots \quad (45).$$

Another disk, attached where z has a finite value, would render the cylinder rigid.

Instead of a plane disk let us next suppose that the cylinder is closed at $z = 0$ by a hemisphere attached to it round the circumference. By (1) the three component displacements at the edge of the hemisphere ($\theta = \frac{1}{2}\pi$) are of the form

$$v = a\,\delta\phi = a \cos s\phi.$$

$$u = a\,\delta\theta = -a \sin s\phi.$$

$$w = \delta r \quad = sa \sin s\phi.$$

Equating these to the corresponding values for the cylinder, as given by (23), (24), (25), we get

$$\mathrm{A}_s = 1, \qquad\qquad \mathrm{B}_s = s;$$

so that the deformation of the cylinder is now limited to the type

$$\left.\begin{aligned} v &= (a+sz) \cos s\phi \\ w &= s\,(a+sz) \sin s\phi \\ u &= -a \sin s\phi \end{aligned}\right\} \quad\ldots\ldots\ldots\ldots\ldots \quad (46),$$

in which we may, of course, introduce an arbitrary multiplier and an arbitrary addition to ϕ. If the convexity of the hemisphere be turned outwards, s is to be considered positive.

In like manner any other convex additions at one end of the cylinder might be treated. There are apparently three conditions to be satisfied by only two constants, but one condition is really redundant, being already secured by the inextensibility of the edges provided for in the types of deformations determined separately for the two shells. Convex additions, closing both ends of the cylinder, render it rigid, in accordance with Jellet's theorem that a closed oval shell cannot be bent.

It is of importance to notice how a cylinder, or a portion of a cylinder, can *not* be bent. Take, for example, an elongated strip, bounded by two generating lines subtending at the axis a small angle. Equations (31) [giving $d^2w/dr^2 = 0$] show that the strip cannot be bent in the plane containing the axis and the middle generating line.* The only bending symmetrical with respect to this

* This is the principle upon which metal is corrugated.

plane is a purely cylindrical one which leaves the middle generating line straight. There are two ways in which we may conceive the strip altered so as to render it susceptible of the desired kind of bending. The first is to take out the original cylindrical curvature, which reduces it to a plane strip. The second is to replace it by one in which the middle line is curved from the beginning, like the equator of a sphere or ellipsoid of revolution. In this case the total curvature being finite, the Gaussian condition can be satisfied by a change of meridianal curvature compensating the supposed change of equatorial curvature. It is easy to calculate the actual stiffness from (8) and (14), for here $\tau = 0$. We have

$$U = \frac{2nh^3}{3}\left(\delta\frac{1}{\rho_1}\right)^2\left\{1+\frac{\rho_1^2}{\rho_2^2}+\frac{m-n}{m+n}\left(1-\frac{\rho_1}{\rho_2}\right)^2\right\} \;\ldots\ldots\; (47),$$

which expresses the work per unit of area corresponding to a given bending $\delta\rho_1^{-1}$ along the equator. If $\rho_1 = \infty$, the cylindrical strip is infinitely stiff. If the curvature be spherical, $\rho_2 = \rho_1$, and

$$U = \frac{4nh^3}{3}\left(\delta\frac{1}{\rho_1}\right)^2 \;\ldots\ldots\ldots\ldots\; (48);$$

and if $\rho_2 = \infty$,

$$U = \frac{4nh^3}{3}\cdot\frac{m}{m+n}\left(\delta\frac{1}{\rho_1}\right)^2 \;\ldots\ldots\ldots\ldots\; (49).$$

Whatever the equatorial curvature may be, the ratio of stiffnesses in the two cases is equal to $m : m+n$, or about $2 : 3$, the spherically curved strip being the stiffer.

The same principle applies to the explanation of Bourdon's gauge. In this instrument there is a tube whose axis lies along an arc of a circle and whose section is elliptical, the longer axis of the ellipse being perpendicular to the general plane of the tube. If we now consider the curvature at points which lie upon the axial section, we learn from Gauss's theorem that a diminished curvature along the axis will be accompanied by a nearer approach to a circular section, and reciprocally. Since a circular form has the largest area for a given perimeter, internal pressure tends to diminish the eccentricity of the elliptic section and with it the general curvature of the tube. Thus, if one end be fixed, a pointer connected with the free end may be made to indicate the internal pressure.*

* Dec. 19.—It appears, however, that the bending of a curved tube of elliptical section cannot be pure, since the parts of the walls which lie furthest from the circular axis are necessarily stretched. The difficulty thus arising may be obviated by replacing the two halves of the ellipse, which lie on either side of the major axis, by two symmetrical curves which meet on the major axis at a *finite angle.*

We will now proceed with the calculation for the frequencies of vibration of the complete cylindrical shell of length $2l$. If the volume-density be ρ,* we have as the expression of the kinetic energy by means of (35), (36), (37).

$$\begin{aligned} T &= \tfrac{1}{2}\,.\,2h\rho\,.\iint (\dot{u}^2+\dot{v}^2+\dot{w}^2)\,a\,d\phi\,dz \\ &= 2\pi\rho hla\,\Sigma\,\{a^2(1+s^2)\,(\dot{A}_s^2+\dot{A}_s'^2) \\ &\qquad +[\tfrac{1}{3}l^2(1+s^2)+s^{-2}a^2]\,(\dot{B}_s^2+\dot{B}_s'^2)\} \quad\ldots\ldots\ldots (50). \end{aligned}$$

From these expressions for V and T in (39), (50) the types and frequencies of vibration can be at once deduced. The fact that the squares, and not the products, of A_s, B_s, are involved, shows that these quantities are really the principal coordinates of the vibrating system. If A_s, or A_s', vary as $\cos p_st$, we have

$$p_s^2 = \tfrac{4}{3}\,\frac{mn}{m+n}\,\frac{h^2}{\rho a^4}\,\frac{(s^3-s)^2}{s^2+1}\quad\ldots\ldots\ldots\ldots\ldots (51).$$

This is the equation for the frequencies of vibration in two dimensions.† For a given material, the frequency is proportional to the thickness and inversely as the square on the diameter of the cylinder.‡

In like manner if B_s, or B_s', vary as $\cos p_s't$, we find

$$p_s'^2 = \tfrac{4}{3}\,\frac{mn}{m+n}\,\frac{h^2}{\rho a^4}\,\frac{(s^3-s)^2}{s^2+1}\,\frac{1+\dfrac{3a^2}{s^2l^2}\,\dfrac{m+n}{m}}{1+\dfrac{3a^2}{(s^4+s^2)\,l^2}}\quad\ldots\ldots (52).$$

If the cylinder be at all long in proportion to its diameter, the

According to the equations (in columnar co-ordinates) of my former paper, the conditions that δr, δz shall be independent of ϕ lead to—

$$\delta r = Cr, \qquad \frac{d\delta z}{dz}+C\left(\frac{dr}{dz}\right)^2 = 0,$$

where C is an absolute constant.

The case where the section is a rhombus ($dr/dz = \pm\tan\alpha$) may be mentioned.

The difficulty referred to above arises when $dr/dz = \infty$.

* This can scarcely be confused with the notation for the curvature in the preceding parts of the investigation.

† See 'Theory of Sound,' § 233.

‡ There is nothing in these laws special to the cylinder. In the case of similar shells of any form, vibrating by pure bending, the frequency will be as the thicknesses and inversely as corresponding areas. If the similarity extend also to the thickness, then the frequency is inversely as the linear dimension, in accordance with the general law of Cauchy.

difference between p_s' and p_s becomes very small. Approximately in this case

$$p_s'/p_s = 1+\frac{3a^2}{2s^2l^2}\left(\frac{m+n}{m}-\frac{1}{s^2+1}\right);$$

or if we take $m = 2n$, $s = 2$,

$$p_2'/p_2 = 1+\frac{7a^2}{20l^2}.$$

In my former paper I gave the types of vibration for a circular cone, of which the cylinder may be regarded as a particular case. In terms of columnar coordinates (z, r, ϕ) we have

$$\delta\phi = (A_s+B_s z^{-1})\cos s\phi \quad\dots\dots\dots\dots\dots\dots\dots\quad (53),$$

$$\delta r = s\tan\gamma\,(A_s z+B_s)\sin s\phi \quad\dots\dots\dots\dots\dots\dots\quad (54),$$

$$\delta z = \tan^2\gamma\,[s^{-1}B_s-s\,(A_s z+B_s)]\sin s\phi \quad\dots\dots\dots\quad (55),$$

γ being the semi-vertical angle of the cone. For the calculation of the energy of bending it would be simpler to use polar coordinates (r, θ, ϕ), r being measured from the vertex instead of from the axis.

If the cone be complete up to the vertex, we must suppose, in (53) &c., $B_s = 0$. And if we proceed to calculate the potential energy, we shall find it infinite, at least when the thickness is uniform. For since A_s is of no dimensions in length, the square of the change of curvature must be proportional to $A_s^2z^{-2}$. When this is multiplied by $z\,dz$, and integrated, a logarithm is introduced, which assumes an infinite value when $z = 0$. The complete cone must therefore be regarded as infinitely stiff, just as the cylinder would be if one rim were held fast.

If two similar cones (bounded by circular rims) are attached so that the common rim is a plane of symmetry, the bending may be such that the common rim remains plane. If the distance of this plane from the vertex be z_1, the condition to be satisfied in (53) &c., is that $\delta z = 0$ where $z = z_1$. Hence

$$\delta\phi = A_s\left\{1-\frac{s^2}{s^2-1}\,\frac{z_1}{z}\right\}\cos s\phi \quad\dots\dots\dots\dots\quad (56),$$

$$\delta r = s\tan\gamma\,A_s\left\{z-\frac{s^2z_1}{s^2-1}\right\}\sin s\phi \quad\dots\dots\dots\quad (57),$$

$$\delta z = s\tan^2\gamma\,A_s\,\{z_1-z\}\sin s\phi \quad\dots\dots\dots\dots\dots\quad (58).$$

III. "An Investigation of a Case of Gradual Chemical Change." By W. H. Pendlebury and M. Seward. Communicated by A. G. Vernon Harcourt, Esq., F.R.S. Received November 27, 1888.

(Abstract.)

The case of gradual chemical change with which the present investigation deals is that between hydrogen chloride and potassium chlorate, and also its reaction with hydrogen chlorate whether alone or in presence of potassium chloride.

When dilute solutions of a chlorate (as for instance potassium chlorate) and hydrogen chloride are mixed together, the liquid slowly acquires a chlorous smell, and there is a gradual liberation of oxidising material, chlorine, and oxides of chlorine. These immediate products cannot easily be investigated, for if the mixture is left to itself so that they accumulate in it, the gradual reaction first observed is stopped, and there ensues decomposition of the usual complex nature of these unstable solutions of chlorine and its oxygenated compounds.

But if a small quantity of potassium iodide is present it will be decomposed by these substances, and iodine will be gradually liberated as the final product of the reaction we have mentioned.

Now Messrs. Harcourt and Esson, in their work on a gradual chemical change, measured the rate at which iodine was liberated in a liquid by ascertaining the time taken for a known quantity of sodium thiosulphate added to that liquid to be entirely decomposed. A small quantity of starch solution was added at the same time, and served as the signal of the presence of free iodine, which meant that the measured quantity of sodium thiosulphate was exhausted. The observation to be made was of the interval of time which elapsed between the addition of the thiosulphate and the first appearance of a blue starch coloration.

The same measurement and the same signal served our purpose. The first obvious difference between the two reactions is that, whereas in the former one (between hydrogen dioxide and hydrogen iodide) iodine was the primary result, in the later one it is a secondary result. This proved an unimportant difference. The secondary reaction between potassium iodide and the results of the first is, by comparison, instantaneous. But another difference is of great importance. In their reaction the rate of decomposition became gradually slower, as one of the substances reacting continued to decrease sensibly in amount, and finally disappeared. In this reaction the amount of each substance decomposed bears an infinitely small

ratio to the amount of each present; the composition of the mixture thus remains practically unchanged, and the rate of decomposition in each mixture is constant. Each experiment then is brought to an arbitrary close as soon as the constant velocity has been determined by the observation of a few intervals. The subjects of investigation were: the comparison of the velocities in different mixtures, and thus the establishment of laws connecting variation in velocity with variation of each of the ingredients.

In a large number of experiments hydrogen chlorate was used. Mixtures of dilute solutions of the two acids, chloric and hydrochloric, were made in various proportions; being arranged in several series in each of which the amount of one of the acids present was varied in arithmetical progression, and the effect upon the rate investigated. Then the effect of the presence of certain quantities of potassium chloride upon the rate was observed, for the purpose of connexion with a new series of experiments. In these potassium chlorate and hydrogen chloride were used, and series for variation of one of the ingredients taken as before. The effect of varying the quantity of potassium iodide present and that of varying the temperatures were also observed.

The results may be thus briefly summarised :—

Variation in Hydrogen Chlorate.—The rate varies with the amount of hydrogen chlorate, in the first place, directly, as a substance taking part in the chemical reaction; and in the second place with a small acceleration proportional to the quantity present, so that the substance has a coefficient of action independent of its being a participant in the reaction. Thus—

$$R = aQ\,(1 + bQ),$$

where Q represents quantity, R rate, a and b constants.

Variation in Hydrogen Chloride.—The variation of the rate with that of hydrogen chloride is not of this simple nature. It would seem to be (1) an effect of the secondary order above mentioned (accelerative) on the decomposition of hydrogen chlorate by itself, and in addition to this (2) an effect of both primary and secondary order on the decomposition of hydrogen chlorate with hydrogen chloride.

Variation in Potassium Chloride.—The addition of this salt has a small accelerative effect on the normal rate proportional to its quantity. It thus appears to be a neutral salt not taking part in the reaction.

If a mixture of solutions of potassium chlorate and hydrogen chloride in molecular proportion between 1 : 2 and 1 : 12 is made, complete double decomposition ensues, the hydrogen chlorate formed, in presence of the hydrogen chloride remaining, liberates oxidising

material, and the potassium chloride formed exercises its specific influence on this reaction.

The secondary action upon potassium iodide producing iodine is practically an instantaneous one, unless the quantity of this substance is below a certain minimum. Below this the velocity observed in the mixture will be less than normal. The effect of increasing the amount of this substance to much greater than the minimum is closely analogous to that of a similar increase of any neutral salt.

The velocity is an exponential function of the temperature, as was observed in Messrs. Harcourt and Esson's investigations. As the latter increases in arithmetical progression, the former increases in geometrical progression. The rate is about doubled for a rise of 5° C. The ratio in this progression is not, however, absolutely constant, but varies a little with the temperature at which it is taken. Thus between 0° and 15° C. the rate is a little more than doubled for a rise of 5°; between 20° and 30° it is a little less than doubled.

IV. "Determination of the Viscosity of Water." By A. MALLOCK. Communicated by Lord RAYLEIGH, Sec. R.S. Received November 30, 1888.

The experiments here described, which were made during April and May of the present year (1888), to determine the constant of viscosity of water, may be of some interest on account of the newness of the method employed, and also as being on rather a larger scale than other experiments which have been made with the same object.

Fig. 1 gives a section of the apparatus used.

A and B are two coaxial cylinders; of these A is mounted on the vertical axis E, and can be made to rotate by a belt passing over the wheel F. B is suspended by a long fine wire C, and the annular space between A and B is filled with water or any other fluid to be experimented on.

A little way above the lower edge of B is fixed an air-tight diaphragm D, so that when the space between the two cylinders is filled with liquid air is inclosed under D, and the liquid touches B only on the cylindrical surface.

The interior of B above D is filled with water which serves the purposes of checking the torsional vibrations of B, of preventing any rapid change of temperature of the liquid in the annulus, and of holding the thermometer.

The experiments were made by driving the cylinder A at a uniform speed and recording the angle through which B is turned when it comes to rest under the action of the fluid friction on its cylindrical surface and the torsion of the suspending wire C. A was driven by

a remontoir weight in connexion with a governor, and the speed recorded electrically on a chronograph by means of a contact maker on the axis E.

The torsion of the wire C was measured on a divided circle H, attached to B.

To get the absolute value of the torsion-scale the following method was used :—

W (fig. 2) is a small weight hung at the end of a silk thread S in the neighbourhood of the torsion-wire C. H is the divided circle on B. From S a second thread, L, is taken to the circumference of H. The point of W is over a horizontal scale, and a reading of its position taken when there is no strain on L, that is, when S is hanging vertically. The weight W is then displaced by unclamping the circle H from B and winding up the thread L round its circumference, then reclamping H and allowing things to come to rest; readings are then taken of the displacement of W and the position of H. After this the thread L is cut and the position of H read again.

These experiments give directly the force which a known angular twist of the wire exerts at the known radius of the divided circle.

These experiments are recorded below, and it will be seen that the results are very fairly consistent.

The dimensions of the various parts of the apparatus are given in the computation of the viscosity constant.

In making the experiments on viscosity the velocity of the circumference of the cylinder A was made to vary from 0·5 to 50 metres per minute.

It was found that at all these speeds the force tending to turn the inner cylinder B could be represented by the sum of two terms, one varying as the velocity and the other as the square of the velocity; the latter being small compared to the former, even at the highest speed. See Diagram 1.

The cause of the square term seems to be that, owing to the action of the bottom of the revolving cylinder, a circulation is set up in the fluid in the annulus, the flow being up the side of the revolving cylinder and down the side of the stationary one, the result being that the fluid having the velocity due to a position near the outer cylinder is by this circulation continuously carried towards the inner one, thus making the variation of velocity in the neighbourhood of the

latter greater than it would otherwise be.* As far as could be observed there was no trace of eddies with axes parallel to that of the cylinders. The proportion between the two terms depends on the ratio between the length of the cylinders and the breadth of the annulus, the square term becoming smaller and smaller compared to the other as the ratio increases.

It was found that when the temperature of the fluid was altered the coefficient of the term varying as the velocity changed, but that the coefficient of the square term remained unaffected.

The value of the viscosity constant deduced from these experiments agrees closely with that obtained from the experiments of Poiseuille on the flow of liquids through capillary tubes.

I now proceed to give the method and the numerical data which were employed in the computation.

Let r_1 = radius of cylinder B . . . . = 4·636
r_2 = " " A . . . . = 5·017
h = depth of immersed surface of B . . . . . = 11·07
v = linear velocity of surface of A,
θ = torsional angle through which B is turned by the action of the water;

$$F = \kappa\,\theta = \kappa\,(Av+Bv^2) = \text{whole tangential force};$$

$$\mu = \text{coefficient of viscosity};$$

the units being the gram, centimetre, and second.

If instead of being in an annulus the water was contained between two parallel planes of infinite extent, the distortion caused by the motion of one of these planes parallel to the other would be uniform throughout the whole mass of enclosed fluid. But in the case of the liquid enclosed between two cylinders, although the distortion is uniform over each cylindrical surface in the fluid coaxial with the enclosing cylinders, yet it changes in passing from one such surface to another, increasing as the radius decreases. In fact, since the total moment transmitted by each surface is constant,† the rate of distortion necessary to produce this moment must be inversely as the area of the surface and radius of the cylinder at which it occurs; that is, the rate of distortion at radius r is proportional to $1/r^2$, hence the value of dv/dr at r is—

* Professor J. Thomson has pointed out that a circulation having a very similar origin must take place in a stream when flowing round a bend.

† [A correction has been introduced here, and in the equations (1), (2), (3). It was originally stated that the force transmitted was constant, but the error was pointed out to me by Lord Rayleigh. In consequence of this error the numerical values of μ subsequently given must be multiplied by 1·08.—January 1, 1889.]

$$\frac{dv}{dr} = \frac{\kappa A v'}{\mu} \frac{r_2}{2\pi r^2 h} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1),$$

where v' is the velocity at r_2.

Integrating between r_1 and r_2 with the conditions that when $r = r$, $v = 0$, and when $r = r_2$, $v = v'$,

$$v' = \frac{\kappa A v'}{\mu} \frac{r_2 - r_1}{2\pi r_1 h} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2),$$

whence

$$\mu = \kappa A \frac{r_2 - r_1}{2\pi r_1 h} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3).$$

The numerical value of A is that of $d\theta/dv$ at the origin of the curve in Diagram 1. The ordinate θ being the circular measure of the angle through which the cylinder B is turned by the viscosity of the water when the cylinder A has the velocity v represented by the abscissa in centimetres per second.

To determine κ the following measures were made:—

In fig. 2 let

$$w = \text{weight of W},$$
$$\text{ED} = b,$$
$$\text{D}'\text{D} = x,$$
$$\text{POP}' = \phi,$$
$$\text{OP} = \text{R}.$$

x is the displacement of w from the vertical caused by the torsion of the wire C through the angle ϕ acting at radius R.

$$\therefore \text{K} \frac{\text{R}}{r_1} \phi = w \frac{x}{b},$$

and

$$\kappa = w \frac{r_1}{\text{R}b} \frac{x}{\phi} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (4).$$

The experiments gave the following values for x and ϕ:—

	x c.m.	ϕ°.	log x/ϕ.
Experiment 1	10·51	316·6	$\bar{2}$·51109
,, 2	8·08	245·4	·51759
,, 3	9·2	276·8	·52162
,, 4	9·88	298·0	·52044
,, 5	10·8	324·0	·52287
,, 6	10·62	321·0	·51961
Mean			·52092

Also	$\log w = 0.81151$	$\log R = 1.06354$	
	$\log r_1 = 0.53705$	$\log b =$	
	1.34856	3.25583	
	$\log Rb = 3.25583$		

$$\begin{array}{rl} & \bar{2}.09273 \\ \log \dfrac{x}{\phi} = & \bar{2}.52092 \\ \hline & \bar{4}.61365 \end{array}$$

Multiplying by 57·3 to convert to circular measure

$$\begin{array}{rl} \log 57.3 = & 1.75815 \\ \hline & \bar{2}.37180 \end{array}$$

Whence $\kappa = 0.02354.$

The diagrams, which were taken at random from many similar ones plotted during the course of the experiments, give A at the temperatures at 4°, 13·8°, and 48° C.

We have
$$A_4 = 0.0582.$$
$$A_{13.8} = 0.0458.$$
$$A_{48} = 0.023.$$

Also since
$$\kappa = 0.02354,$$
$$r_2 = 5.017,$$
$$h = 11.07,$$
$$r_2 - r_1 = 0.381,$$

we have
$$\frac{\kappa(r_2-r_1)}{2\pi r_2 h} = 2.606\times 10^{-5},$$

whence
$$\mu_4 = 15.166\times 10^{-7},$$
$$\mu_{13.8} = 11.93 \;\ldots\; ,,$$
$$\mu_{48} = 5.99 \;\ldots\; .$$

The results are shown in the form of a curve in Diagram 2, the ordinates being the values of μ_r and the abscissæ the temperature.

Poiseuille's results are shown by the dotted curve.

The chief interest of these experiments, beyond that attaching to an independent determination of μ by a new method, lies in the comparatively high velocities at which the viscous forces remain the principal cause of resistance.

In all other experiments on fluid friction with which I am acquainted (those on capillary tubes excepted) the term depending on the square of the velocity becomes the most important at speeds far below those used in this series.

Many experiments were made on the viscosity of fluids other than water, but as I find that the results do not differ materially from those of Poiseuille it is unnecessary to give them here.

DIAGRAM 1.

DIAGRAM 2.

Presents, December 13, 1888.

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December 20, 1888.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Co-relations and their Measurement, chiefly from Anthropometric Data." By FRANCIS GALTON, F.R.S. Received December 5, 1888.

"Co-relation or correlation of structure" is a phrase much used in biology, and not least in that branch of it which refers to heredity, and the idea is even more frequently present than the phrase; but I am not aware of any previous attempt to define it clearly, to trace its mode of action in detail, or to show how to measure its degree.

Two variable organs are said to be co-related when the variation of the one is accompanied on the average by more or less variation of the other, and in the same direction. Thus the length of the arm is said to be co-related with that of the leg, because a person with a long arm has usually a long leg, and conversely. If the co-relation be close, then a person with a very long arm would usually have a very long leg; if it be moderately close, then the length of his leg would usually be only long, not very long; and if there were no co-relation at all then the length of his leg would on the average be mediocre. It is easy to see that co-relation must be the consequence of the variations of the two organs being partly due to common causes. If they were wholly due to common causes, the co-relation would be perfect, as is approximately the case with the symmetrically disposed parts of the body. If they were in no respect due to common causes, the co-relation would be *nil.* Between these two extremes are an endless number of intermediate cases, and it will be shown how the

closeness of co-relation in any particular case admits of being expressed by a simple number.

To avoid the possibility of misconception, it is well to point out that the subject in hand has nothing whatever to do with the average proportions between the various limbs, in different races, which have been often discussed from early times up to the present day, both by artists and by anthropologists. The fact that the average ratio between the stature and the cubit is as 100 to 37, or thereabouts, does not give the slightest information about the nearness with which they vary together. It would be an altogether erroneous inference to suppose their average proportion to be maintained so that when the cubit was, say, one-twentieth longer than the average cubit, the stature might be expected to be one-twentieth greater than the average stature, and conversely. Such a supposition is easily shown to be contradicted both by fact and theory.

The relation between the cubit and the stature will be shown to be such that for every inch, centimetre, or other unit of absolute length that the cubit deviates from the mean length of cubits, the stature will on the average deviate from the mean length of statures to the amount of 2·5 units, and in the same direction. Conversely, for each unit of deviation of stature, the average deviation of the cubit will be 0·26 unit. These relations are not numerically reciprocal, but the exactness of the co-relation becomes established when we have transmuted the inches or other measurement of the cubit and of the stature into units dependent on their respective scales of variability. We thus cause a long cubit and an equally long stature, as compared to the general run of cubits and statures, to be designated by an identical scale-value. The particular unit that I shall employ is the value of the probable error of any single measure in its own group. In that of the cubit, the probable error is 0·56 inch = 1·42 cm.; in the stature it is 1·75 inch = 4·44 cm. Therefore the measured lengths of the cubit in inches will be transmuted into terms of a new scale, in which each unit = 0·56 inch, and the measured lengths of the stature will be transmuted into terms of another new scale in which each unit is 1·75 inch. After this has been done, we shall find the deviation of the cubit as compared to the mean of the corresponding deviations of the stature, to be as 1 to 0·8. Conversely, the deviation of the stature as compared to the mean of the corresponding deviations of the cubit will also be as 1 to 0·8. Thus the existence of the co-relation is established, and its measure is found to be 0·8.

Now as to the evidence of all this. The data were obtained at my anthropometric laboratory at South Kensington. They are of 350 males of 21 years and upwards, but as a large proportion of them were students, and barely 21 years of age, they were not wholly full-grown; but neither that fact nor the small number of observations is

prejudicial to the conclusions that will be reached. They were measured in various ways, partly for the purpose of this inquiry. It will be sufficient to give some of them as examples. The exact number of 350 is not preserved throughout, as injury to some limb or other reduced the available number by 1, 2, or 3 in different cases. After marshalling the measures of each limb in the order of their magnitudes, I noted the measures in each series that occupied respectively the positions of the first, second, and third quarterly divisions. Calling these measures in any one series, Q_1, M, and Q_3, I take M, which is the median or middlemost value, as that whence the deviations are to be measured, and $\frac{1}{2}\{Q_3-Q_1\} = Q$, as the probable error of any single measure in the series. This is practically the same as saying that one-half of the deviations fall within the distance of $\pm Q$ from the mean value, because the series run with fair symmetry. In this way I obtained the following values of M and Q, in which the second decimal must be taken as only roughly approximate. The M and Q of any particular series may be identified by a suffix, thus M_c, Q_c might stand for those of the cubit, and M_s, Q_s for those of the stature.

Table I.

	M.		Q.	
	Inch.	Centim.	Inch.	Centim.
Head length...........	7·62	19·35	0·19	0·48
Head breadth..........	6·00	15·24	0·18	0·46
Stature................	67·20	170·69	1·75	4·44
Left middle finger......	4·54	11·53	0·15	0·38
Left cubit.............	18·05	45·70	0·56	1·42
Height of right knee....	20·50	52·00	0·80	2·03

NOTE.—The head length is its maximum length measured from the notch between and just below the eyebrows. The cubit is measured with the hand prone and without taking off the coat; it is the distance between the elbow of the bent left arm and the tip of the middle finger. The height of the knee is taken sitting when the knee is bent at right angles, less the measured thickness of the heel of the boot.

Tables were then constructed, each referring to a different pair of the above elements, like Tables II and III, which will suffice as examples of the whole of them. It will be understood that the Q value is a universal unit applicable to the most varied measurements, such as breathing capacity, strength, memory, keenness of eyesight, and enables them to be compared together on equal terms notwithstanding their intrinsic diversity. It does not only refer to measures of

length, though partly for the sake of compactness, it is only those of length that will be here given as examples. It is unnecessary to extend the limits of Table II, as it includes every line and column in my MS. table that contains not less than twenty entries. None of the entries lying within the flanking lines and columns of Table II were used.

Table II.

Stature in inches.	Length of left cubit in inches, 348 adult males.								Total cases.
	Under 16·5.	16·5 and under 17·0.	17·0 and under 17·5.	17·5 and under 18·0.	18·0 and under 18·5.	18·5 and under 19·0.	19·0 and under 19·5.	19·5 and above.	
71 and above	..	..	..	1	3	4	15	7	30
70...........	..	..	..	1	5	13	11	..	30
69...........	..	1	1	2	25	15	6	..	50
68...........	..	1	3	7	14	7	4	2	48
67...........	..	1	7	15	28	8	2	..	61
66...........	..	1	7	18	15	6	..	..	48
65...........	..	4	10	12	8	2	..	..	36
64...........	..	5	11	2	3	..	..	..	21
Below 64.....	9	12	10	3	1	..	..	..	34
Totals	9	25	49	61	102	55	38	9	348

The measures were made and recorded to the nearest tenth of an inch. The heading of 70 inches of stature includes all records

Table III.—Stature M_s = 67·2 inches; Q_s = 1·75 inch. Left Cubit M_c = 18·05 inches; Q_c = 0·56 inch.

No. of cases.	Stature.	Deviation from M_s reckoned in		Mean of corresponding left cubits.	Deviation from M_c reckoned in			Smoothed values multiplied by Q_c.	Added to M_c.
		Inches.	Units of Q_s.		Inches.	Units of Q_c. Observed.	Units of Q_c. Smoothed.		
	inches.			inches.					
30	70·0	+2·8	+1·60	18·8	+0·8	+1·42	+1·30	+0·73	18·8
50	69·0	+1·8	+1·03	18·3	+0·3	+0 53	+0·84	+0·47	18·5
38	68·0	+0·8	+0·46	18 2	+0·2	+0·36	+0·38	+0·21	18·3
61	67·0	−0·2	−0·11	18 1	+0·1	+0·18	−0·08	−0·04	18·0
48	66·0	−1·2	−0·69	17·8	−0·2	−0·36	−0·54	−0·30	17·8
36	65·0	−2·2	−1·25	17·7	−0·3	−0·53	−1·00	−0·56	17 5
21	64·0	−3·2	−1·83	17·2	−0·8	−1·46	−1·46	−0·80	17·2

No. of cases.	Left cubit.	Deviation from M_c reckoned in		Mean of corresponding statures.	Deviation from M_s reckoned in			Smoothed values multiplied by Q_s.	Added to M_s.
		Inches.	Units of Q_c.		Inches.	Units of Q_s. Observed.	Units of Q_s. Smoothed.		
	inches.			inches.					
38	19·25	+1·20	+2·14	70·3	+3 1	+1·8	+1·70	+3·0	70·2
55	18·75	+0·70	+1·25	69·7	+1·5	+0 9	+1·00	+1·8	69·0
102	18·25	+0 20	+0·36	67·4	+0·2	+0 1	+0·28	+0·5	67·7
61	17·75	−0·30	−0·53	66·3	−0·9	−0·5	−0·43	−0·8	66·4
49	17·25	−0·80	−1·42	65 0	−2·2	−1·3	−1·15	−2·0	65·2
25	16·75	−1·30	−2·31	63·7	−3·5	−2·0	−1·85	−3·2	64·0

between 69·5 and 70·4 inches; that of 69 includes all between 68·5 and 69·4, and so on.

The values derived from Table II, and from other similar tables, are entered in Table III, where they occupy all the columns up to the three last, the first of which is headed "smoothed." These smoothed values were obtained by plotting the observed values, after transmuting them as above described into their respective Q units, upon a diagram such as is shown in the figure. The deviations of the "subject" are measured parallel to the axis of y in the figure, and those of the mean of the corresponding values of the "relative" are measured parallel to the axis of x. When the stature is taken as the subject, the median positions of the corresponding cubits, which are given in the successive lines of Table III, are marked with small circles. When the cubit is the subject, the mean positions of the corresponding statures are marked with crosses. The firm line in the figure is drawn to represent the general run of the small circles and crosses. It is here seen to be a straight line, and it was similarly found to be straight in every other figure drawn from the different pairs of co-related variables that I have as yet tried. But the inclination of the line to the vertical differs considerably in different cases. In the present one the inclination is such that a deviation of 1 on the part of the subject, whether it be stature or cubit, is accompanied by a mean deviation on the part of the relative, whether it be cubit or stature, of 0·8. This decimal fraction is consequently the measure of the closeness of the co-relation. We easily retransmute it into inches. If the stature be taken as the subject, then Q_s is associated with $Q_c \times 0{\cdot}8$; that is, a deviation of 1·75 inches in the one with $0{\cdot}56 \times 0{\cdot}8$ of the other. This is the same as 1 inch of stature being associated with a mean length of cubit equal to 0·26 inch. Conversely, if the cubit be taken as the subject, then Q_c is associated with $Q_s \times 0{\cdot}8$; that is, a deviation of 0·56 inch in the one with $1{\cdot}75 \times 0{\cdot}8$ of the other. This is the same as 1 inch of cubit being associated with a mean length of 2·5 inches of stature. If centimetre be read for inch the same holds true.

Six other tables are now given in a summary form, to show how well calculation on the above principle agrees with observation.

Table IV.

No. of cases.	Length of head.	Mean of corresponding statures. Observed.	Mean of corresponding statures. Calculated.
32	7·90	68·5	68·1
41	7·80	67·2	67·8
46	7·70	67·6	67·5
52	7·60	66·7	67·2
58	7·50	66·8	66·8
34	7·40	66·0	66·5
26	7·30	66·7	66·2

No. of cases.	Height.	Mean of corresponding lengths of head. Observed.	Mean of corresponding lengths of head. Calculated
26	70·5	7·72	7·75
30	69·5	7·70	7·72
50	68·5	7·65	7·68
49	67·5	7·65	7·64
56	66·5	7·57	7·60
43	65·5	7·57	7·69
81	64·5	7·54	7·65

No. of cases.	Height.	Mean of corresponding lengths of left middle finger. Observed.	Mean of corresponding lengths of left middle finger. Calculated.
30	70·5	4·71	4·74
50	69·5	4·55	4·68
37	68·5	4·57	4·62
62	67·5	4·58	4·56
48	66·5	4·50	4·50
37	65·5	4·47	4·44
20	64·5	4·33	4·38

No. of cases.	Length of left middle finger.	Mean of corresponding statures. Observed.	Mean of corresponding statures. Calculated
23	4·80	70·2	69·4
49	4·70	68·1	68·5
62	4·60	68·0	67·7
63	4·50	67·3	66·9
57	4·40	66·0	66·1
35	4·30	65·7	65·3

No. of cases.	Left middle finger.	Mean of corresponding lengths of left cubit. Observed.	Mean of corresponding lengths of left cubit. Calculated
23	4·80	18·97	18·80
50	4·70	18·55	18·49
62	4·60	18·24	18·18
62	4·50	18·00	17·87
57	4·40	17·72	17·55
34	4·30	17·27	17·24

No. of cases.	Length of left cubit.	Mean of corresponding length of left middle finger. Observed.	Mean of corresponding length of left middle finger. Calculated
29	19·00	4·76	4·75
32	18·70	4·64	4·69
48	18·40	4·60	4·62
70	18·10	4·56	4·55
37	17·80	4·49	4·48
31	17·50	4·40	4·41
28	17·20	4·37	4·34
24	16·90	4·32	4·28

Table IV—*continued.*

No. of cases.	Length of head.	Mean of corresponding breadths of head.		No. of cases.	Breadth of head.	Mean of corresponding lengths of head.	
		Observed.	Calculated.			Observed.	Calculated.
32	7·90	6·14	6·12	27	6·30	7·72	7·84
41	7·80	6·05	6·08	36	6·20	7·72	7·75
46	7·70	6·14	6·04	53	6·10	7·65	7·65
52	7·60	5·98	6·00	58	6·00	7·68	7·60
58	7·50	5·98	5·96	56	5·90	7·50	7·55
34	7·40	5·96	5·91	37	5·80	7·55	7·50
26	7·30	5·85	5·87	30	5·70	7·45	7·46
No. of cases.	**Stature.**	**Mean of corresponding heights of knee.**		**No. of cases.**	**Height of knee.**	**Mean of corresponding statures.**	
		Observed.	Calculated.			Observed.	Calculated.
30	70·0	21·7	21·7	23	22·2	70·5	70·6
50	69·0	21·1	21·3	32	21·7	69·8	69·6
38	68·0	20·7	20·9	50	21·2	68·7	68·6
61	67·0	20·5	20·5	68	20·7	67·3	67·7
49	66·0	20·2	20·1	74	20·2	66·2	66·7
36	65·0	19·7	19·7	41	19·7	65·5	65·7
				26	19·2	64·3	64·7
No. of cases.	**Left cubit.**	**Mean of corresponding heights of knee.**		**No. of cases.**	**Height of knee.**	**Mean of corresponding left cubit.**	
		Observed.	Calculated.			Observed.	Calculated
29	19·0	21·5	21·6	23	22·25	18·98	18·97
32	18·7	21·4	21·2	30	21·75	18·68	18·70
48	18·4	20·8	20·9	52	21·25	18·38	18·44
70	17·1	20·7	20·6	69	20·75	18·15	18·17
37	17·8	20·4	20·2	70	20·25	17·75	17·90
31	17·5	20·0	19·9	41	19·75	17·55	17·63
28	17·2	19·8	19·6	27	19·25	17·02	17·36
23	16·9	19·3	19·2				

From Table IV the deductions given in Table V can be made; but they may be made directly from tables of the form of Table III, whence Table IV was itself derived.

When the deviations of the subject and those of the mean of the relatives are severally measured in units of their own Q, there is always a regression in the value of the latter. This is precisely

Table V.

Subject.	Relative.	In units of Q.		In units of ordinary measure.	
		r.	$\sqrt{(1-r^2)} = f$.	As 1 to	f.
Stature	Cubit	0·8	0·60	0·26	0·45
Cubit..........	Stature			2·5	1·4
Stature	Head length.....	0·35	0·93	0·38	1·63
Head length....	Stature			3·2	0·17
Stature	Middle finger....	0·7	0·72	0·06	0·10
Middle finger...	Stature			8·2	1·26
Middle finger...	Cubit	0·85	0·61	3·13	0·34
Cubit..........	Middle finger....			0·21	0·09
Head length....	Head breadth....	0·45	0·89	0·43	0·16
Head breadth...	Head length.....			0·48	0·17
Stature	Height of knee ..	0·9	0·44	0·41	0·35
Height of knee .	Stature			1·20	0·77
Cubit..........	Height of knee ..	0·8	0·60	1·14	0·64
Height of knee .	Cubit			0·56	0·45

analogous to what was observed in kinship, as I showed in my paper read before this Society on "Hereditary Stature" ('Roy. Soc. Proc.,' vol. 40, 1886, p. 42). The statures of kinsmen are co-related variables; thus, the stature of the father is correlated to that of the adult son, and the stature of the adult son to that of the father; the stature of the uncle to that of the adult nephew, and the stature of the adult nephew to that of the uncle, and so on; but the index of co-relation, which is what I there called "regression," is different in the different cases. In dealing with kinships there is usually no need to reduce the measures to units of Q, because the Q values are alike in all the kinsmen, being of the same value as that of the population at large. It however happened that the very first case that I analysed was different in this respect. It was the reciprocal relation between the statures of what I called the "mid-parent" and the son. The mid-parent is an ideal progenitor, whose stature is the average of that of the father on the one hand and of that of the mother on the other, after her stature had been transmuted into its male equivalent by the multiplication of the factor of 1·08. The Q of the mid-parental statures was found to be 1·2, that of the population dealt with was 1·7. Again, the mean deviation measured in inches of the statures of the sons was

found to be two-thirds of the deviation of the mid-parents, while the mean deviation in inches of the mid-parent was one-third of the deviation of the sons. Here the regression, when calculated in Q units, is in the first case from $\frac{1}{1\cdot 2}$ to $\frac{2}{3}\times 1\cdot 7 = 1$ to 0·47, and in the second case from $\frac{1}{1\cdot 7}$ to $\frac{1}{3}\times\frac{1}{1\cdot 2} = 1$ to 0·44, which is practically the same.

The *rationale* of all this will be found discussed in the paper on "Hereditary Stature," to which reference has already been made, and in the appendix to it by Mr. J. D. Hamilton Dickson. The entries in any table, such as Table II, may be looked upon as the values of the vertical ordinates to a surface of frequency, whose mathematical properties were discussed in the above-mentioned appendix, therefore I need not repeat them here. But there is always room for legitimate doubt whether conclusions based on the strict properties of the ideal law of error would be sufficiently correct to be serviceable in actual cases of co-relation between variables that conform only approximately to that law. It is therefore exceedingly desirable to put the theoretical conclusions to frequent test, as has been done with these anthropometric data. The result is that anthropologists may now have much less hesitation than before, in availing themselves of the properties of the law of frequency of error.

I have given in Table V a column headed $\sqrt{(1-r^2)} = f$. The meaning of f is explained in the paper on "Hereditary Stature." It is the Q value of the distribution of any system of x values, as x_1, x_2, x_3, &c., round the mean of all of them, which we may call X. The knowledge of f enables dotted lines to be drawn, as in the figure above, parallel to the line of M values, between which one half of the x observations, for each value of y, will be included. This value of f has much anthropological interest of its own, especially in connexion with M. Bertillon's system of anthropometric identification, to which I will not call attention now.

It is not necessary to extend the list of examples to show how to measure the degree in which one variable may be co-related with the combined effect of n other variables, whether these be themselves co-related or not. To do so, we begin by reducing each measure into others, each having the Q of its own system for a unit. We thus obtain a set of values that can be treated exactly in the same way as the measures of a single variable were treated in Tables II and onwards. Neither is it necessary to give examples of a method by which the degree may be measured, in which the variables in a series each member of which is the summed effect of n variables, may be modified by their partial co-relation. After transmuting the separate measures as above, and then summing them, we should find the probable error of any one of them to be $\sqrt{n}$ if the variables were

perfectly independent, and n if they were rigidly and perfectly co-related. The observed value would be almost always somewhere intermediate between these extremes, and would give the information that is wanted.

To conclude, the prominent characteristics of any two co-related variables, so far at least as I have as yet tested them, are four in number. It is supposed that their respective measures have been first transmuted into others of which the unit is in each case equal to the probable error of a single measure in its own series. Let y = the deviation of the subject, whichever of the two variables may be taken in that capacity; and let x_1, x_2, x_3, &c., be the corresponding deviations of the relative, and let the mean of these be X. Then we find: (1) that $y = r\text{X}$ for all values of y; (2) that r is the same, whichever of the two variables is taken for the subject; (3) that r is always less than 1; (4) that r measures the closeness of co-relation.

II. "On the Maximum Discharge through a Pipe of Circular Section when the effective Head is due only to the Pipe's Inclination." By Henry Hennessy, F.R.S., Professor of Applied Mathematics in the Royal College of Science for Ireland. Received November 15, 1888.

In the paper on "Hydraulic Problems on the Cross-sections of Pipes and Channels,"* it was shown that the greatest hydraulic mean depth was that for a channel formed by a segment of a circle, and bounded by an arc of 257° 27′. It is easy to find by a similar process the wetted perimeter of a circular pipe corresponding to the maximum discharge when the velocity of the liquid is due only to the inclination of the pipe.

Among the formulæ adopted by hydraulic engineers for v, the mean velocity of liquid in a pipe whose hydraulic mean depth is u, we may select Darcy's, which gives

$$v^2 = \frac{u\text{I}}{a+\dfrac{b}{u}},$$

where a and b are constant coefficients and I a quantity depending on the inclination of the pipe. But as the discharge Q is the product of the mean velocity by the area of cross-section, we have

$$\text{Q} = \frac{\text{A}u\sqrt{(\text{I})}}{\sqrt{(au+b)}} = \frac{\frac{1}{2}r^2(\theta-\sin\theta)u\sqrt{(\text{I})}}{\sqrt{(au+b)}},$$

* 'Roy. Soc. Proc.,' vol. 44, p. 101.

where θ is the arc bounding the segment filled with liquid. For this segment $u=\frac{1}{2}r\left(1-\frac{\sin\theta}{\theta}\right)$, and therefore

$$Q=\frac{1}{2\sqrt{(2)}}\,\frac{r^{\frac{5}{2}}}{a^{\frac{1}{2}}}\,\frac{I^{\frac{1}{2}}}{\theta^{\frac{1}{2}}}\,\frac{(\theta-\sin\theta)^2}{\sqrt{\left(\theta-\sin\theta+\frac{2b}{ra}\right)}}.$$

b is a small fraction compared to r and a, and if this expression is developed we shall have very approximately*

$$Q=K\frac{(\theta-\sin\theta)^{\frac{3}{2}}}{\theta^{\frac{1}{2}}},$$

where K is a constant. This gives

$$\frac{1}{K}\frac{dQ}{d\theta}=\frac{3}{2}\frac{(\theta-\sin\theta)^{\frac{1}{2}}(1-\cos\theta)}{\theta^{\frac{1}{2}}}-\frac{(\theta-\sin\theta)^{\frac{3}{2}}}{2\theta^{\frac{3}{2}}}.$$

If we make $\frac{dQ}{d\theta}=0$, we shall have therefore

$$\theta=\frac{\sin\theta}{3\cos\theta-2}.$$

This equation may be satisfied by $\theta=0$, or $\theta=\frac{3}{2}\pi+\gamma$, a value less than 2π. The first gives a minimum, the second a maximum.

With $\gamma=38°\ 9'\ 56''$, $\frac{3}{2}\pi+\gamma=5\cdot37850$,

$$3\sin\theta=1\cdot85381,\quad \frac{\cos\theta}{2-3\sin\theta}=5\cdot37813.$$

With $\gamma=38°\ 9'\ 57''$, $\frac{3}{2}\pi+\gamma=5\cdot37851$,

$$3\sin\theta=1\cdot85382,\quad \frac{\cos'\theta}{2-3\sin\theta}=5\cdot37848.$$

With $\gamma=38°\ 9'\ 58''$, $\frac{3}{2}\pi+\gamma=5\cdot37851$,

$$3\sin\theta=1\cdot85583,\quad \frac{\cos\theta}{2-3\sin\theta}=5\cdot37882.$$

With the first value the difference is $+0\cdot00037$; with the third the difference is $-0\cdot00031$; consequently the value between both may be considered as the nearest to the truth, and in this value the difference is only $0\cdot00003$, or less than one-tenth of either of the others. If $\gamma=38°\ 9'\ 57''$, $\theta=308°\ 9'\ 57''$, or a circular pipe, under the conditions above mentioned, carries more liquid when filled up to this arc than when quite full.

* With the formulæ of Chezy and Eytelwein, this would immediately follow.

If the pipe was quite full $\theta = 2\pi$, $\sin\theta = \theta$, and $Q_1 = 2K\pi$, but for the maximum value of Q we have

$$Q_2 = K\left\{\frac{(5{\cdot}37851+0{\cdot}61794)^3}{5{\cdot}337851}\right\}^{\frac{1}{2}}.$$

Hence

$$\frac{Q_2}{Q_1} = \frac{1}{2\pi}\left\{\frac{(5{\cdot}99646)^3}{5{\cdot}33785}\right\}^{\frac{1}{2}} = 1{\cdot}00768.$$

$$Q_2 - Q_1 = 0{\cdot}00768\,Q_1.$$

The difference thus exceeds ¾ per cent. for the pipe which is filled up to the segment of 308° 10′. The supplemental arc being 51° 50′, it is easy to see that the maximum discharge would occur when the liquid falls below the summit of the inner surface of the pipe by about the twentieth of the diameter. This result might be called a hydraulic paradox, or the condition of a pipe carrying liquid at a small inclination giving a greater discharge when filled up to nineteen-twentieths of its diameter than when completely full.

Note added December 19, 1888.

[The hydraulic paradox here referred to as a deduction from the expression for hydraulic mean depth is not so practically important as the question of velocity of the liquid passing through the section of greatest hydraulic mean depth. The maximum hydraulic mean depth for the pipe was found to be $0{\cdot}6086r$, while it is $0{\cdot}5r$ for a full pipe. As the velocities may be taken as very approximately proportional to the square roots of the hydraulic mean depths, we shall have for v', the maximum velocity,

$$v' = v\sqrt{\frac{6086}{5000}} = 1{\cdot}1033\,v.$$

Or the velocity for the maximum hydraulic depth exceeds the velocity for a full pipe under the conditions specified by 10⅓ per cent.

This result may possibly be utilised in circular drain-pipes liable to be coated with deposits.]

III. "Preliminary Account of the Morphology of the Sporophyte of *Splachnum luteum*." By J. R. VAIZEY, M.A., of Peterhouse, Cambridge. Communicated by FRANCIS DARWIN, F.R.S. Received December 3, 1888.

The investigations of Haberlandt,* published in the latter part of 1886, together with the results of investigations of my own, which were then just completed, and communicated to the Linnean Society† early in 1887, convinced me of the importance of obtaining further knowledge of the highest development to which the sporophyte of the mosses attains, as being likely to throw light indirectly on the phylogeny of the higher Cryptogams and Phanerogams. Inquiring into the matter, I found that *Splachnum luteum*, *Splachnum rubrum*, and some few other forms were the most likely to yield the best results; I determined, therefore, to obtain material for investigating their morphology. These forms being arctic or subarctic, I put myself in communication with Professor Axel Blytt, of Christiania, to find out if he could either procure me material properly preserved for the purpose, or put me in the way of obtaining material if I went myself to Norway. From my correspondence with Professor Blytt, I concluded that the only really practicable course was to go myself, and obtain my own material in the different stages in which I required it. To carry out this project, I applied for, and was granted, assistance by the Royal Society. I, therefore, now tender to the Society a brief outline of the first of my results.

I obtained after considerable search, in which I was fortunate in having the invaluable assistance of Professor Blytt and Dr. F. C. Kiaer, whose knowledge of the habitats of Norwegian mosses is notorious, a large quantity of *Sp. luteum* in many different stages of development; of *Sp. rubrum* I only obtained one specimen; but beyond the mere difference of colour there is little or no difference between the two species. The material was obtained in the marshy land on the top of the watershed between the River Glommen and Lake Miosen, and on the south-eastern side of the Dovrefjeld region.

In the sporophyte of *Splachnum luteum* we have a structure with a remarkable similarity to an umbrella, the handle end of which is inserted in the tissues of the oophyte, and is known as the foot. The seta is much elongated, bearing the umbrella-like expansion, the apophysis, at the top just below the sporangium. It is the structure

* "Beiträge zur Anatomie und Physiologie der Laubmoose;" 'Jahrb. für Wissen. Bot.,' vol. 17.

† Vaizey: "On the Anatomy and Development of the Sporogonium of the Mosses." 'Linn. Soc. Journ., Bot.,' vol. 24.

of the apophysis and certain of the organs of the sporophyte with which we are now concerned.

A transverse section through the vaginula, including the foot of the sporophyte, shows that the tissues of the oophyte in this part contain a considerable quantity of organic substance, and this is seen to be more particularly the case in the layers of cells next to the foot. The foot itself is seen to consist of a cylindrical mass of parenchyma, with an external layer of epidermal cells of a somewhat columnar form, which contain a considerable quantity of protoplasm, and contain large distinct nuclei. The protoplasm of these cells is found to be aggregated towards the peripheral surface, the nucleus being usually found in the mass of protoplasm next to the outer wall of the cell. The large vacuoles of these cells are traversed by fine protoplasmic filaments. These cells, as well as those of the cortical layer beneath the epidermis, contain a number of very small protoplasmic bodies, which are found congregated in large numbers round the nuclei of the cells, there being also some in other parts of the cell, both in the peripheral layer and in the fine protoplasmic filaments traversing the vacuole. In the epidermal cells these bodies are particularly numerous, and are found principally in the aggregated mass of protoplasm on the outer side of the cells. These bodies may, I think, be safely regarded as leucoplastids. From their number and position, I am inclined to believe that they are concerned in absorbing substances from the tissue of the oophyte for the nourishment of the sporophyte. No starch has been found in the foot.

In the centre of the foot there is a definite central strand consisting of two kinds of tissue, an outer phloëm-like layer of cells containing protoplasm by means of which it is probable that organic substance travels, and an inner strand of very thin-walled cells without any protoplasmic contents* which conducts the water up the seta. In the foot the protoplasm of the phloëm-like cells is aggregated in each cell towards the periphery as in the epidermal cells, but there are no plastids present. The strand of thin-walled empty cells† I have been able to prove in other species of *Splachnum* conveys the water absorbed by the foot up the seta into the tissues of the apophysis.

The seta has a distinct epidermis beneath which there is a layer of sclerotic supporting tissue, and then a layer of parenchyma, the two together forming the cortex. In the centre is the central strand, which in the lower end of the seta has almost the same structure as that described for the central strand of the foot, from which it is distinguished by being larger and less distinctly delimited from the

* *Cf.* Vaizey, *loc. cit.* The terms leptophloëm and leptoxylem have been used to indicate these tissues. For fuller explanation, see paper referred to.

† Vaizey: "Note on the Transpiration of the Sporophore of the Musci." 'Annals of Botany,' vol. 1.

cortex. Higher up in the seta there is a large intercellular canal formed in the middle of the axile strand of thin-walled empty cells which extends for nearly its whole length. This intercellular space is lysigenous in origin. A similar passage or canal occurs in several other species.

A longitudinal median section through the umbrella-shaped apophysis shows that the central strand here swells out into a large pear-shaped mass of cells, that in the mature sporophyte contain no protoplasm, and even in the younger states only a very small quantity with small, inconspicuous nuclei. Chlorophyll bodies are absent except in the two outermost layers of cells, even in the youngest specimens observed, and even here there are only a very few. The cells are all thin-walled, and cubical in shape, with no intercellular spaces between them. In this tissue, which may be regarded as a kind of aqueous tissue, large masses of crystalline inorganic matter were frequently found.

Outside the aqueous tissue there is a quantity of parenchymatous tissue, with numbers of communicating intercellular spaces. The cells all contain large numbers of chlorophyll bodies. This tissue extends into the umbrella-shaped organ. On the upper surface in the proximal region the cells are arranged close to one another, and show a distinct tendency to an elongation of their axes in a direction vertical to the surface, thus forming a palisade tissue similar to that in the tissues of the vascular plants.* This is rendered more striking by a comparison with the parenchyma of the lower surface in the same region, where the cells are much elongated in a direction parallel to the surface, and with very much larger intercellular spaces. The distal region of the apophysis shows that the cells of both upper and lower surfaces have undergone a considerable lengthening in the direction parallel to the surfaces, but that the upper as compared with the lower has still a resemblance to palisade. Stomata are found in considerable numbers in the epidermis of the upper surface, but there are none on the lower. The epidermis consists of a very distinct layer of cells without chlorophyll, the outer walls of which are cuticularised, and have a distinct cuticle.

A large quantity of starch is formed in the cells of the apophysis by the chloroplasts, each chloroplast containing a number of separate starch grains. When the apophysis is quite young, at this time being green, immediately on its beginning to become umbrella-shaped, and before the spores ripen, the starch begins to be formed. At a later stage the starch disappears, the starch-forming plastids,

* Haberlandt (*loc. cit.*) also makes a comparison between the chlorophyll-containing tissue of the sporophyte of the Mosses and the palisade tissue of true leaves; but in none of the forms which he investigated is this structure as striking as it is in *S. luteum*.

which before were large and well formed, degenerate into small and comparatively inconspicuous bodies, the starch apparently being used up in the formation of spores. In all probability there is at this period a formation of xanthophyll, which would account for the yellow colour of the apophysis in the mature condition of the sporangium, and hence the name of the species.

That the apophysis performs the functions of a leaf, and is therefore *analogous* with the leaves of vascular plants, I think there can now be no doubt. And as this structure is a development of the sporophyte, the possibility of its being also *homologous* either directly or indirectly suggests itself. I am myself inclined to believe that the two are homologous; but to give a full discussion of that question would be beyond the scope of the present communication.

IV. "A Contribution to the Knowledge of Protection against Infectious Diseases." By ALFRED LINGARD, M.B., M.S. Durh., Diplomate in Public Health, Cambridge. Communicated by Dr. E. KLEIN, F.R.S. Received December 3, 1888.

It has long been known, and it is now a well-established fact, that various eruptive fevers and blood diseases from which the mother may suffer, can be communicated to the fœtus *in utero*. There is evidence also to prove that a disease may be transmitted to the fœtus through a mother who is herself insusceptible to contagium, as in the case of a child having been born covered with small-pox eruption, the mother being quite free from it. The following are the diseases upon which the most important observations have been made:—Syphilis, small-pox, tuberculosis, anthrax, and relapsing fever. In the three latter the organisms producing these diseases have been found in the body of the fœtus at birth, having passed through the placental vessels.

In the present paper I wish to contribute to the other side of the question, viz., the relation existing between the fœtus and its mother, or, in other words, *the influence, if any, exerted by the fœtus on the mother, when the fœtus becomes the subject of an infectious disease contracted independently of the mother.* All the comments made from this standpoint have, with the exception of one, been in relation to syphilis; the one being an instance communicated by Vidal, of a father attacked at the time of conception with small-pox, the fœtus at six months being covered, during the whole of which period the mother remained healthy. With regard to syphilis, we are indebted to Colles for the first practical observation noted in 1837, when he cited as a curious fact, that he had never witnessed or even heard of an instance in which a child deriving the infection of syphilis from its *parents*, had caused an ulceration in the breast of the mother.

At the present time, however, we are able to go a step farther, and say—

(1.) That a healthy woman become pregnant by a syphilitic man, may give birth to a syphilitic child, and still remain healthy herself.

(2.) That this woman suckling a syphilitic child is not exposed to contagion from it.

This singular immunity remains only to be explained, and we have to determine whether it is not explicable, as one is led to think, by a special kind of protection derived from the fœtus.

Several years ago it occurred to me as feasible to attempt the elucidation of this proposition by means of some virus other than that of syphilis, this disease having been found incapable of communication to the lower animals. For this purpose none appeared to be more suitable than that of anthrax, on account of the properties and life-history of this organism being so well understood, and also by reason of the very short period of time this disease takes to run its course to a fatal termination after inoculation in most of the lower animals.

The results of this investigation I propose giving in the following pages:—

I. It is possible to directly inoculate a fœtus *in utero* of a living rabbit with an active growth of anthrax, without the bacillary disease being communicated to the mother; and further, the remaining fœtuses of this pregnancy under certain conditions have been found to receive a like protection. A control animal subcutaneously inoculated with the same growth died in sixty-eight hours.

II. The mother may give birth to a litter of healthy young ones some days later, with the exception of the one primarily inoculated with anthrax, which is always dead when born. The longest period of parturition after inoculation was ten days.

III. The blood of the mother during the time intervening between the inoculation of the fœtus and parturition does not reveal the presence of the anthrax bacillus when examined—

(i.) By fresh cover-glass preparations.

(ii.) By aniline stained cover-glass preparations.

(iii.) By cultivations, gelatine at 21° C., and agar-agar at 37° C.

(iv.) By symptoms when animals were inoculated with it.

IV. The mother subsequently inoculated with the blood of an animal dead of anthrax, whose blood was swarming with the *Bacillus anthracis*, does not succumb, but is found to have received protection. The control animal died in forty-eight hours.

V. Twenty-four hours after this second inoculation to prove protection or otherwise, no anthrax bacilli were found in the blood of the mother. Proved as in No. III.

VI. The same animal, when re-inoculated with the anthrax blood eight months later, was proved to be still protected.

VII. The shortest period observed intervening between the inoculation of the fœtus *in utero* and parturition, after which the mother was found to be protected against the inoculation of virulent anthrax blood, was thirty-six hours.

VIII. For the protection of the surviving fœtuses, or those other than the one primarily inoculated with anthrax *in utero*, a longer exposure is required than the minimum thirty-six hours observed to protect the mother. Or the surviving fœtuses may have received protection, provided that a period of not less than six days have elapsed between the primary inoculation of the fœtus *in utero* and parturition.

IX. In those cases where the mother died of anthrax contracted at the time of the inoculation of the fœtus *in utero*, and excepting the last-mentioned one, the heart's blood of the other fœtuses *in utero* was not found to contain any anthrax bacilli, as proved by cultivations when the examination was made, several hours after the death of the mother. But if the examination and cultivations were made some sixty or seventy hours later, then any or all of the fœtuses, according to the temperature of the air prevailing, may have anthrax bacilli in their blood.

[X. The inoculation of a fœtus *in utero* with anthrax may produce one of three results:—

(i.) If during the inoculation of a fœtus the anthrax bacilli gain entrance into the tissues of the mother, owing to imperfect manipulation, the mother naturally succumbs to the disease.

(ii.) In some cases the organisms pass through from the fœtal to the maternal vessels; this is probably due to some change taking place in the placental tissues, either inflammatory or traumatic in origin.

(iii.) Lastly, in those cases where the fœtus alone is inoculated, the mother remains free from the bacillary disease, and at a later date is found to have acquired immunity.—Jan. 22, 1889.]

XI. In sections of the placenta of the fœtus primarily inoculated with anthrax *in utero*, and through which the mother received protection, the anthrax bacilli, after staining with aniline dyes, are to be seen wholly in the fœtal, while there is a total absence of them in the maternal portion.

The Society adjourned over the Christmas Recess to Thursday, January 10th, 1889.

Presents, December 20, 1888.

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January 10, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Paper was read:—

"APPENDIX TO THE BAKERIAN LECTURE,* SESSION 1887-88." By J. NORMAN LOCKYER, F.R.S. Received November 22, 1888.

CONTENTS.

* The title of the Bakerian Lecture was:—"Suggestions on the Classification of the various Species of Heavenly Bodies." A Report to the Solar Physics Committee. Communicated at the request of the Committee.

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Comet Wells.
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III. Light curves.
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VI. Binary Stars, Class 3.—Equal or Nearly Equal Magnitudes, one Star being Blue.

INTRODUCTION.

In the Bakerian Lecture given last Session* I detailed the spectroscopic evidence which in my opinion shows that the various orders of nebulæ and stars are produced by the presence and subsequent condensation of meteoric swarms in space, the most uncondensed ones giving rise to the appearances which we term nebulæ, the more condensed ones to those which we term stars.

Since the lecture was delivered, my assistants and myself have been employed not only in continuing the experiments, but in bringing together and co-ordinating as great a number of recorded observations as possible, along those lines which seemed likely to furnish the most severe tests as to the validity of the conclusions stated in my former communications.

Among the lines on which this work has been done are the following:—

1. *Spectra of Comets.*—Here the test is as follows:—It is generally accepted that comets are meteor-swarms in the solar system. They get brighter, and therefore they must be hotter, as they approach the sun. Their spectra, then, if my hypothesis is true, must resemble those of gradually condensing swarms outside the system.

2. *Spectra of Aurora.*—Here the test is as follows:—400,000,000 meteorites, big and little, are encountered by the earth every day. The air should contain some of their *débris*. If in auroræ the solid particles are acted on by an electric current, the spectral phenomena presented by glow tubes should be reproduced to a greater or less extent in the spectrum of the aurora.

3. *Origin of Double Stars.*—Here the test is as follows:—The apparently single variable stars of the Mira type are on the hypothesis produced by the interaction of two or more swarms; they are in fact double nebulæ. Visible physical doubles are probably then of the same nature; if so, in the present absence of complete knowledge of their spectra, colour phenomena may help us to discuss their probable origin.

* See 'Roy. Soc. Proc.,' vol. 44, p. 1.

I. "ON THE SPECTRA OF METEORIC SWARMS IN THE SOLAR SYSTEM."

I. Views of Reichenbach, Schiaparelli, and Tait.

Reichenbach was the first to bring forward a large amount of evidence (founded on the study of meteorites) indicating that comets were in all probability swarms of meteorites* *in our own system* moving in orbits round the sun.

Accepting as proved by the then knowledge the most intimate connexion between meteorites and falling stars, Reichenbach reasoned that both were connected with comets in the following manner. He first recapitulated the facts then accepted with regard to comets:—

(1.) Comets, both tail and nucleus are transparent.

(2.) Light is transmitted through comets without refraction; hence the cometary substance can be neither gaseous nor liquid.

(3.) The light is polarised, and therefore borrowed from the sun.

(4.) Comets have no phases like those of moon and planets.

(5.) They exercise no perturbing influences.

(6.) Donati's comet (which was then visible) in its details and its contour is changing every day—according to Piazzi, almost hourly.

(7.) The density of a comet is extremely small.

(8.) The absolute weight is sometimes small (von Littrow having calculated the masses of very small comets, tail and all, as scarcely reaching 8 lbs).

From these data the following conclusions might be drawn:—

(1.) That a comet's tail must consist of a swarm of extremely small but solid particles, therefore granules.

(2.) That every granule is far away from its neighbour—in fact, so far that a ray of light may have an uninterrupted course through the swarm.

(3.) That these granules, suspended in space, move freely and yield to outer and inner agencies—agglomerate, condense, or expand; that a comet's nucleus, where one is present, is nothing else than such an agglomeration of loose substances consisting of particles.

Hence we must picture a comet as a loose, transparent, illuminated, free-moving swarm of small solid granules suspended in empty space.

The next step in Reichenbach's reasoning was to show that meteorites (of which he had a profound knowledge) were really composed of granules.

He pointed out that these granules (since called chondroi) formed really the characteristic structure both of irons and stones, so that both orders were chiefly aggregates of chondroi—stony ones in iron meteorites, iron ones in stony meteorites.

* Poggendorff, 'Annalen,' vol. 105, 1858, p. 438.

In some irons, such as Zacatecas, they exist as big as walnuts, firmly adherent, but they can be separated; inside these are balls of troilite often firmly embedded, so that on breaking the metoorite they will divide, but in other cases so loose that they fall out, and they are smooth enough to roll off a table.

Sometimes chondroi have smaller ones sprinkled in them, sometimes dark chondroi have white earthy kernels.

In some cases these chondroi are so plentiful as to form nearly the whole mass of the meteorite. They are often perfectly round, but not always, and they are so often so loose that they tumble out and leave an empty smooth spherical cavity.

The stones chiefly consist of such chondroi and their *débris*.

He adds that each magnetic chondros "is an independent crystallised individual—it is a stranger in the meteorite. Every chondros was once a complete, independent, though minute meteorite. It is embedded like a shell in limestone. Millions of years may have passed between the formation of the spherule and its embeddal."

He finally remarks that the chondroi of meteorites indicate a condensation of innumerable bodies such as we see must exist in the case of comets; further, that they have been formed in a state of unrest and impact from all sides. Many meteorites are true breccias; they have *many times* suffered mechanical violence: in comets we have seen precisely the conditions where such forces could operate, and hence he arrives at the view that "comets and meteorites may be nothing else but one and the same phenomenon."

Schiaparelli* in 1886 showed the probability that comets, with which he had identified certain recurring streams of shooting stars, were swarms of meteorites drawn from the depths of space by the attraction of the outer planets of the solar system or by the general attraction of the system itself.

Schiaparelli did not look upon the head of a comet as a swarm of meteors as Reichenbach did, but regarded it as the largest meteorite in the stream which produced the star-shower. "Nous voici donc arrivés à cette conséquence véritablement inattendue, que la grande comète de 1862 n'est autre qu'une des Perséides du mois d'Août, et c'est probablement la plus considérable de toutes." †

Professor Tait in 1869, supporting the opinion of Reichenbach, showed that the cometary phenomena to which Reichenbach had called attention could be mechanically explained by the assumption of a cloud of meteorites.

He writes: "The principal object of the paper is to investigate how far the singular phenomena exhibited by the tails of comets, and by the envelopes of their nuclei, the shrinking of their nuclei as they

* 'Les Mondes,' vols. 12 and 13, 1886.

† Schiaparelli, 'Les Mondes,' vol. 13, p. 76, 1867.

approach the sun, and *vice versâ*, as well as the diminution of period presented by some of them, can be explained on the probable supposition that a comet is a mere cloud of small masses such as stones and fragments of meteoric iron, shining by reflected light alone, except where these masses impinge on one another, or on other matter circulating round the sun, and thus produce luminous gases, along with considerable modifications of their relative motions. Thus the gaseous spectrum of the nucleus was assigned to the same impacts which throw out from the ranks those masses which form the tail."*

It is not too much to say that at the present time it is generally accepted that the heads of comets are meteor-swarms, possibly the densest portion of each swarm, or portions with the same orbit in the case of multiple comets.

I propose now to set forth the spectroscopic evidence which I have obtained bearing upon the nature of, and the changes which take place in, these meteoric swarms which have become entangled in our system.

II. Comets at Aphelion. Lowest Temperature.

Magnesium Radiation, λ 500.

When a tube such as I have already described is used in experiments to determine the spectrum of meteoric dust at the lowest temperature, we find that the dust in many cases gives a spectrum containing the magnesium fluting at 500, which is characteristic of the nebulæ, and is often seen alone in them. If the difference between nebulæ and comets is merely of cosmographical position, one being out of the solar system, and one being in it, and further, if the conditions as regards rest are the same, the spectrum should be the same, and we ought to find this line in the spectrum of comets, when the swarm most approaches the undisturbed nebulous condition, the number of collisions being at or near a minimum, *i.e.*, when the comet is near aphelion, the fluting should be visible alone.

As a matter of fact in comets of 1866 and 1867, when they were observed away from the sun, the only line seen was the one at 500.†

It is probable also that the fourth band mentioned by Konkoly in

* Tait, 'Edinb. Roy. Soc. Proc.,' vol. 6, p. 553 (1869).

† "In January, 1866, I communicated to the Royal Society the result of an examination of a small comet visible in the beginning of that year ('Roy. Soc. Proc.,' vol. 15, p. 5). I examined the spectrum of another small and faint comet in May, 1867. The spectra of these objects, so far as their feeble light permitted them to be observed, appeared to be very similar. In the case of each of these comets the spectrum of the minute nucleus appeared to consist of a bright line between *b* and F, about the position of the double line of the spectrum of nitrogen, while the nebulosity surrounding the nucleus and forming the coma gave a spectrum which was apparently continuous" (Huggins, 'Roy. Soc. Proc.,' vol. 16, p. 381).

his observations on the Great Comet (*b*) 1882 (date of perihelion passage September 27th) on November 1st, was the low-temperature fluting of magnesium at 500. By that date the D line and the carbon flutings had passed their maximum intensity, and had begun to fade out.

The same fluting was also seen by Vogel in Coggia's Comet (IV, 1874) as a bright line at about 499, when the comet was yet a month from perihelion, and when therefore the appearance of the low-temperature characteristic of the magnesium spectrum would be expected.

It is fair to myself to say that I was not aware of these observations when I began my recent researches. The fact of the line at 500 remaining alone in Nova Cygni, however, made it clear that if my views were correct, the same thing should happen with comets. It now turns out that the crucial observation which I intended to make was made more than twenty-two years ago.

This spectroscopic evidence is of the strongest, but it does not stand alone; comets at aphelion present the telescopic appearance for the most part of globular nebulæ.

If it be taken as generally accepted that comets are of nebulous origin, it must be remembered that there are no visible nebulæ near enough to our system to supply this material. Prior, therefore, to the effects produced by solar or planetary attraction, the material was in a state of repose; *there were no collisions, and therefore no luminosity.* It is not surprising, then, that the faintest comets and the faintest nebulæ should both, as a rule, be of globular form.

Carbon Radiation.

It is well known that comets generally give us the spectrum of carbon at some time or another on their journey to and from the sun. The question arises, is there any evidence that when at some distance from the sun the carbon phenomena observed indicate a low-temperature? Is the presence of low-temperature magnesium associated with low-temperature flutings of carbon?

In my paper* of November 17th, 1887, I gave a map showing the two sets of flutings, and one to show low and high temperatures. The brightest edges of the three principal flutings in the low-temperature spectrum are at wave-lengths 519·7, 560·7, and 483·3, and those in the high-temperature spectrum are at 516·4, 563·3, and 473·6. The two first flutings in each of the two spectra fall pretty near to those in the other, and a considerable degree of accuracy, which has not in a great number of cases been attained in the observations of cometary bands, is therefore necessary before we can say with abso-

* 'Roy. Soc. Proc.,' vol. 43, p. 132.

lute certainty from observations of either of these two bands whether the spectrum is that of hot or cool carbon.

If, however, the fluting at 483 is present, we can be certain that we have to deal with cool carbon, because no hot carbon fluting falls near that wave-length. In laboratory experiments with Geissler tubes, the passage from one spectrum to the other is very gradual, so that it is not uncommon to have the two spectra superposed, and we might therefore expect a reproduction of this in cometary spectra, and I have no doubt that the changes from the cool to the hot carbon spectrum are answerable for many of the apparent discrepancies in different observations of the same comet, as I pointed out in November, 1887.

There is another difficulty which must not be passed over; individual observations have not in all cases been recorded. Observers have in many cases been in the habit of giving the means of their several observations, and hence the differences in wave-length of the flutings due to the changes from cool to hot carbon, or *vice versâ*, if they exist, cannot be certainly followed in many cases.

A discussion of all the recorded observations at my disposal, however, shows that in some comets we have distinct evidence of cool carbon flutings, but as happens with the magnesium fluting at λ 500, the observations recording them are comparatively few. The reason is probably the same in both cases, namely, that the temperature being low, the light is consequently excessively feeble, and observations are very difficult.

We have evidence of cool carbon in Winnecke's Comet, 1868 (perihelion passage, June 25th). On the 17th June, M. Wolf* recorded three flutings, the wave-lengths of which, as determined by a curve, are about 480, 517, and 560. These differ from their equivalents in the cool carbon spectrum by almost equal amounts, so there can be little doubt that the comet's spectrum was that of cool carbon.

At the return of this comet in 1877, cool carbon was again observed when it was about a month from perihelion.† The perihelion passage occurred on April 17th, and the observation was made on May 15th. Two bands were measured, one at 517, and the other near 483. Another was also seen near 561. As the criterion for cool carbon is the fluting at 483, there can be no doubt of its identity in this case.

Again, in Brorsen's Comet (1879), perihelion passage 30th March, Konkoly‡ observed three flutings at wave-lengths 482·3, 514·6, and 560·5, the first of which coincides very nearly with the characteristic

* 'Comptes Rendus,' vol. 66, p. 1336.

† 'Greenwich Observations,' 1887, p. 101.

‡ 'Astr. Nachr.,' No. 2269.

fluting of cool carbon at 483. This observation was made on the 25th of March.

III. Comets about Mean Distance—2nd Stage of Heat.

When meteorite dust is more strongly heated in a glow tube, the whole tube, when the electric current is passing, gives us the fluted spectrum of carbon, and other bright metallic flutings are added to that of magnesium at 500. Among those metallic flutings which are first added may be chiefly mentioned Mg 5210 and Mn (1) 558.

Both these as well as the high-temperature fluting of carbon, have been seen in comets, and I now proceed to give the details of the observations.

Magnesium Radiation, 5210.

While comets at their lowest temperatures give the magnesium fluting at 500, as they approach perihelion, to this is added the fluting at 5210. The result when this is seen with the 517 fluting of carbon,

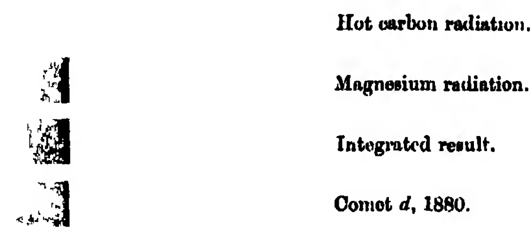


Fig. 1.—*Diagram showing the result of the integration of the hot carbon fluting at 517 and the magnesium fluting at 521, compared with Comet d, 1880.*

which is always present, is an apparent displacement of the carbon fluting to a less refrangible position as shown in fig. 1. This probably occurred in the following comets:—

Wave-length.	Name of Comet.	Date of observation.	P.P.	P.D.	Observer.
520·1	*d* 1880	7 Oct., 1880	6 Sept.	..	Christie.*
520·0	III 1881	27 June, 1881	16 June	0·7345	Hasselberg.†

* 'Astron. Soc. Monthly Notices,' vol. 41, p. 58.

† Pamphlet. 'Mém. de l'Acad. de St. Pétersbourg,' vol. 28, No. 2.

Wı	Nı of ı		P.P.	P.D.		
475, 513, 555	I, 1864...	Aug. 5, 1864	Aug. 15, 1864	0·90929	Donati	'Astr. Nachr.,' No. 1488.
473, 516, 553	n's, 1868...	April 29, 1868	April 20, 186[illegible]	0·596762	Secchi	'C R.,' vol. 66, p. 882.
473, 512, 557	's IV, 1871	Nov. 11, 1871	Nov. 30, 1871	1·03011	Vogel	'Bothk. Beob.,' p. 62.
474, 516, 564	's V, 1871	„ 8, 1871	Dec. 28, 1871	0·332875	Huggins	'Roy. Soc. Proc.,' vol. 20, p. 45.
473, 516, 552	IV, 1873	..	Sept. 10, 187[illegible]	0·7940	Vogel	'Astr. Phys. Obs.,' vol. 11, p. 180.
472, 515, 564	's III, 1874	June 15, 1874	July 8, 187[illegible]	0·6757	„	Do. do.
470, 514, 558	IV, 1874	Sept. 7, 1874	Aug. 27, 187[illegible]	0·9826	Konkoly	'Spect der Cometen,' p. 60.
476, 517, 556	„ I, 1877 ..	Mar. 2, 1877	Jan. 19, 187[illegible]	0·8074	Secchi	Do. do. p 63.
472, 516, 556	Winnecke's, 1877	April 18, 1877	April 17, 187[illegible]	0·8499	Copeland	'Monthly Not.,' vol. 37, p 430.
470, 516, 546	Brorsen's, 1879..	April 16 .... May 2 & 3 ..	March 30, 187[illegible]	0·589892	ped. & Lohse	'Monthly Not.,' vol. 40, p. 23.
474, 517, 564	Comet III, 1881	June 28, 1881	June 16, 188[illegible]	0·7345	Copeland	'Copernicus,' vol. 2, p. 227.
Band, 516, Band	Wells I, 1882 ...	April 6 & 12 May 12 & 22	June 10, 188[illegible]	0·06076	Vogel	'Astr. Nachr.,' No. 2434.
471, 516, 562	Gt. C[illegible]t of 188[illegible]	Nov. 6–18....	Sept. 17, 188[illegible]	0·007753	Gothard	'Astr. Nachr.,' No. 2716.

It will be seen that in each of these cases the observations were made when the comets were at a considerable distance from perihelion, when the temperature would not be very high, although higher than that which gives Mg 500.

Carbon Radiation.

When a comet gets nearer the sun there is a change in its spectrum similar to that observed in the experimental tube at the second stage of heat. Not only does the magnesium radiation change, as we have seen, but the spectrum of carbon, produced from some compound of carbon or another, in nineteen cases out of twenty when the comet gets nearer the sun, and near enough to the earth to be satisfactorily observed, becomes most prominent.

Under these conditions, under which comets generally lend themselves best to spectroscopic study, the spectrum consists chiefly therefore of the flutings of hot carbon. In the majority of cases the spectrum of a comet has not been recorded until it has arrived at this stage of temperature.

The three chief flutings of hot carbon have their least refrangible maxima at approximately 517, 564, and 474. The accompanying table indicates some of the comets in which they have been observed. The variations in the position of the citron band will be again referred to.

It is necessary to state that the maximum luminosity of the blue band, under some conditions, is at about 468. As I have so often had occasion to refer to this, I here reproduce (fig. 2) one of the

1.

2.

3.

FIG. 2.—Spectra of Alcohol at different Pressures.

1. Highest pressure. 2. Lower pressure. 3. Lowest pressure.

many photographs of the spectra of carbon compounds which show it. The diagram is taken from a photograph of the spectrum of alcohol vapour in a capillary tube with a 9-inch spark.

The conditions under which this band has its maximum luminosity at 468 in Geissler tubes seem to be those of maximum conductivity. If the pressure be high all the members of the group are sharp, and the luminosity of the band is almost uniform throughout. This always occurs when the pressure is very low. At intermediate stages of pressure, however, the luminosity of the band has a very decided maximum at about 468.

This latter condition has been reproduced in many comets, though generally the band has been stated to end at 474, or thereabouts, the maximum possibly having been overlooked.

It seems probable that a detailed study of this band in our laboratories will enable us in the future to determine the approximate temperature of a comet by the appearance of this band in its spectrum.

In the spectrum of Comet *b*, 1881 (Observation, June 28th, P.P. June 16, 'Copernicus,' vol. 2, p. 227), Copeland states that this band has a fairly sharp edge at 474, and a maximum at 468.

To measure a maximum in any band is at all times difficult—and extremely so in the cases of cometary spectra—and Copeland says of the above comet:—"The spectrum seemed to change in intensity from moment to moment like a dancing aurora borealis."

The following table includes the above case, and gives also two other comets in which the blue band had the same appearance:—

Edge of band.	Maximum of band.	Name of comet.	When observed.	P.P.	P.D.	Observer.
473	469	Coggia's III, 1874	4 June, 1874	8 July, 1874	0·6757	Vogel.*
473	468	Comet III, 1881	28 June, 1881	16 June, 1881	0·7345	Copeland.†
474	470	Comet IV, 1881	22 Aug., 1881	22 Aug., 1881	0·6311	Copeland.†

The Irregularities Observed in the Citron Fluting.

It has long been known that the least refrangible band in cometary spectra shows great variation in position from the edge of the true citron carbon-band at 564, and many of these variations have been

* 'Astr. Phys. Obs.,' vol. 2, p. 180. † 'Copernicus,' vol. 2, p. 227.

attributed to faulty observation; but this is certainly not so in all

The following, which I quote from Dr. Copeland's discussion of observations on comet spectra, is important in its bearing upon this point:—"We cannot omit to say a few words about the first—yellowish-green band. It is generally described as similar to the two other bands, beginning brightest towards the red, and fading gradually away towards the violet. It is true the dispersive power of the instrument greatly modifies the appearance, but we must say, that under high dispersion we have never seen the first band like the others: it always faded away on both sides, and had seldom a very marked maximum, sometimes it had two, and, perhaps, more, and it seems to be the only band which shows an essentially different appearance in different comets, and, therefore, deserves always a special examination. Unfortunately, it is nearly always the faintest band, and difficult to deal with, and only in Comet III, 1881, traces of what may be bright lines were recognisable; that the iron lines have any connexion with it is very doubtful, since E falls outside of it."*

Again, Professor Young remarks:—

"It is hardly necessary to say that the evidence as to the identity of the flame and comet spectrum is almost overwhelming. The peculiar, ill-defined appearance of the cometary bands at the time of the comet's greatest brightness is, however, something which I have not succeeded in imitating with the flame spectrum. The comet spectrum on July 25th certainly presented a general appearance quite different from that of the later observations as regards the definition of the bands."†

Other observers have also remarked this variability in the *citron* band.

A discussion of the recorded observations shows that this variability is perfectly regular, and depends chiefly on the distance of the comet from perihelion. When carbon first makes its appearance in the spectrum as the comet approaches the sun, the wave-length of the citron band agrees with that of the carbon fluting at 564. As the comet gets nearer perihelion the changes begin, and I now proceed to show that the irregularities are produced by a special case of masking due to the addition of the radiation of manganese or of manganese and lead.

In the Bakerian Lecture (page 63) I showed that in the spectra of some "stars" the characteristics of the spectra of many substances are considerably modified by what I called "masking." Thus in the early species of Group II we have manganese indicated, not by the first fluting at 558, but by the second at 586. This is due to the

* 'Copernicus,' vol. 2, p. 243.

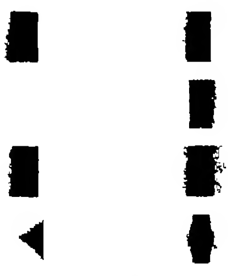
† 'Amer. Journ. Sci.,' 3 series, vol. 22, p. 157.

masking effect of the bright carbon fluting beginning at 564. The radiation of manganese, and sometimes of lead, is added to that of carbon, since the first fluting (558) of manganese falls in the carbon band; the result is a new band of a different form. A further complication, as we shall see, is added when lead, as well as manganese, makes its appearance.

The addition of the manganese radiation does not take place in all comets at an equal number of days from the perihelion passage; it depends upon the perihelion distance, so that the irregularities in question are not observed in all comets.

Manganese Radiation.

When we deal with the integration of the bright manganese fluting at 558, which fades away towards the red, and the carbon fluting at 564, fading towards the blue, we have as a result a band brightest in the centre and fading off in both directions. If both flutings are well developed there will be a single broad maximum extending from 558 to 564, as shown in fig. 3. If both were rather



Hot carbon radiation.

Manganese radiation.

Integrated result.

Great Comet 1882 (Copeland).

FIG. 3.—Diagram showing the result of the integration of hot carbon (517) and manganese (558) radiation, compared with the Great Comet of 1882.

feeble there would be two maxima, one at 558 and one at 564; but this condition has not yet been recorded.

In the Great Comet of 1882, when at a considerable distance from the sun, on October 22nd, the perihelion passage occurring on September 17th, the broad maximum condition, as shown in fig. 3, was recorded by Copeland.

This also occurred in the following comets:—

Wave-lengths.	Name of comet.	Observer.	Date of observation.	P.P.	P.D
556	Encke's V, 1871....	Vogel* ..	11 Nov., '71	28 Dec., '71	0·332875
558	Comet IV, 1874....	Konkoly†	7 Sept., '74	27 Aug., '74	0·9826
556	„ I, 1877	Secchi‡ ..	2 Mar., '77	19 Jan, '77	0·8074
558	Winnecke's, 1877 ..	Copeland§	5 May, '77	17 Apr., '77	0·007753
557	Great Comet of 1882	Copeland‖	Oct. 22, 23	17 Sept., '82	0·9499

Lead Radiation.

When to the radiation of carbon and manganese that of lead is added (546 fluting), three maxima are seen, as shown in fig. 4.

Fig. 4.—Diagram showing the result of the integration of hot carbon, manganese, and lead radiations, compared with the Spectrum of Comet III, 1881.

This condition has been recorded in two comets, as in the following table:—

Wave-lengths.	Name of comet.	Observer.	Date of observation.	P.P.	P.D.
563, 556, 546	Comet III, 1881	Copeland¶	27 July ..	16 June, '81	0·7345
561, 557, 544	Comet IV, 1881	Copeland**	22 August	22 August ..	0·6311

* 'Bothk. Beob.,' vol. 1, p. 60.

† 'Spect. der Cometen,' p. 60.

‡ 'Spec. der Cometen,' p. 61.

§ 'Monthly Notices,' vol. 37, p. 482.

‖ 'Copernicus,' vol. 2, p. 241.

¶ 'Copernicus,' vol. 2, p. 225.

** 'Copernicus,' vol. 2, p. 238.

IV. The Stage Immediately Preceding Perihelion.

Manganese Absorption.

It has been pointed out that in the case of a comet approaching perihelion, manganese is first represented by the radiation of the fluting at 558. As the comet gets nearer to perihelion, if the perihelion distance be sufficiently small, we find the radiation of manganese replaced by absorption.

The reason that the presence of the strongest manganese fluting at 558 has not been previously recorded is, I fancy, that the masking effects of one spectrum on another, to which I referred in the Bakerian Lecture, have not been present in the minds of even those observers who were familiar with low-temperature spectra.

I have obtained abundant evidence that the masking phenomena manifest themselves in the spectra of comets, but since there is in general so little continuous spectrum to be absorbed (from which we can gather that the meteorites are farther apart in comets at this stage than they are in many stars of Group II), we have chiefly to deal, when discussing absorption, with the masking of the radiating citron fluting of carbon by the absorption of metallic vapours.

The way in which the manganese absorption shows itself in comets is generally by the obliteration of the red end of the citron fluting, which produces an apparent shifting of the carbon fluting towards the more refrangible part of the spectrum. The way in which this comes about is shown in fig. 5. The manganese absorption masks the

Hot carbon radiation.

Manganese absorption.

Integrated result

Comet III, 1868.

Fig. 5.—Diagram showing the result of the integration of hot carbon radiation and manganese absorption, compared with Comet III, 1868.

brightest part of the carbon fluting, leaving a sharp edge at 558. This has been observed in eight comets when not far from perihelion, namely:—

Wave-lengths.	Name of comet.	Observer.	Date of observation.	P.P.	P.D.
555	Comet I, 1864	Donati* ..	Aug. 6....	Aug. 15..	0·90929
559	Winnecke's III, 1868	Huggins†	June 22 ..	June 26 ..	0·781538
558	Tuttle's IV, 1871....	Vogel‡....	Nov. 13 ..	Nov. 30..	1·03011
559	Encke's V, 1871 ...	Young§ ..	Dec. 1....	Dec. 28 ..	0·332875
557	Coggia's III, 1874 ..	Vogel‖....	June 7....	June 8 ..	0·6757
556	Winnecke's, 1877....	Copeland¶	April 18 ..	April 17..	0·9499
559	Palisa's *d*, 1879	Konkoly**	Oct. 6	Oct. 8....	0·9896
558	Wells's I, 1882......	Copeland††	May 28 ..	June 10..	0·06076
557	Great Comet II, 1882	Copeland‡‡	Sept. 18 ..	Sept. 17..	0·007783

The result is an apparent displacement of the 564 fluting, whilst the 517 fluting retains its position. This is by far the most general case of masking in comets.

D'Arrest ('Astr. Nachr.,' No. 2001, p. 138), speaking of Coggia's Comet, says:—"The centre shows a bright continuous spectrum with some dark absorption bands." This observation was made on June 15th, and the perihelion passage of the comet took place on July 8th, 1874. The statement is so indefinite, however, that to determine the origin of the bands is almost out of the question. It is probable that one of the bands at least was due to manganese. The above view is strengthened by the fact that Vogel's observation on June 15th ('Astr. Nachr.,' vol. 85, p. 19) gave indications of manganese absorption.

There is another interesting point in connexion with manganese. In the second part of this Appendix I show that the principal aurora line (557) is in all probability the remnant of the manganese fluting at 558, and hence there is a close relation between the spectrum of the aurora and cometary spectra. Professor Young recognised this relation as far back as 1872, but he attached no importance to it. In a note on Encke's Comet§§ he states that, "Although quite probably merely accidental, it may be also worth noting that the principal line of the aurora spectrum (wave-length 5568) very closely coincides with the lowest (cometary) band."

Lead Absorption.

In other cases we have, in addition to the absorption of manganese, the absorption of the lead fluting at 546. The result of this is a much greater apparent shifting of the carbon fluting at 564, as shown in fig. 6. In the absence of the carbon fluting 564, which is not so

* 'Spectra der Cometen,' p. 24.
† 'Phil. Trans.,' vol. 158, p. 556.
‡ 'Bothk. Beob.,' vol. 1, p. 62.
§ 'Amer. Journ. Sci.,' vol. 3, p. 81.
‖ 'Astr. Nachr.,' vol. 85, p. 12.
¶ 'Monthly Notices,' vol. 37, p. 432.
** 'Astr. Nachr.,' vol. 92, p. 301.
†† 'Copernicus,' vol. 2, p. 223.
‡‡ 'Copernicus,' vol. 2, p. 223.
§§ 'Amer. Journ.,' vol 3, Feb., 1872.

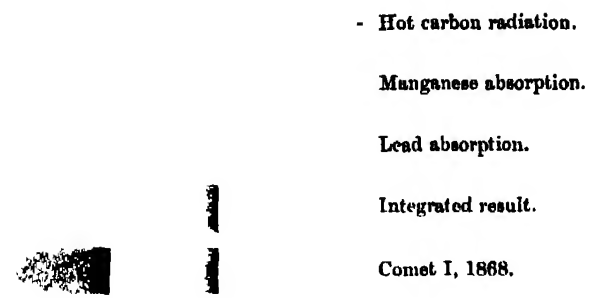


Fig. 6.—Diagram showing the result of the integration of hot carbon radiation and the absorption of manganese and lead, compared with Comet I, 1868.

persistent as the one at 517, we should still get pretty nearly the same result by contrast; that is, the darkening due to absorption commencing at 545 would give rise to an apparent bright fluting at 546, fading away on the more refrangible side. This occurred in the following comets:—

Wave-lengths.	Name of comet.	Observer.	Date of observation.	P.P.	P.D.
544	Brorsen's I, 1868 ...	Huggins*	April 29 ..	April 20..	0·598762
546·8	Wells's I, 1882......	Copeland†	June 1	June 10 ..	0·06076
547·4	Great Comet II, 1882	Copeland‡	Sept. 18 ..	Sept. 17..	0·007753
547·6	Brorsen's *a*, 1879....	Copeland§	April 2....	March 30	0·589

It is important to note, as a test of the validity of this explanation, that the lead fluting never occurs without the manganese one, otherwise we should get two bright maxima, one at 564, and the other at 546.

In the case of Comet III, 1881, it seems probable that both the first and second flutings of lead were absorbing. Copeland ('Copernicus,' vol. 2, p. 226) states that on June 25th, there was a dark band at 567·9. The perihelion passage of the comet occurred on June 16th, and the band was not seen in its spectrum on any other occasion.

There can be little doubt that the band at 567·9 was due to lead

* 'Roy. Soc. Proc.,' vol. 16, p. 386.

† 'Copernicus,' vol. 2, p. 233.

‡ 'Copernicus,' vol. 2, p. 237.

§ 'Monthly Notices,' vol. 39, p. 420.

(λ = 568). The amount of lead in the comet was probably small, and the first band at 546 was evidently masked by the bright carbon fluting observed on the same date. The diminution in brightness of the comet as it receded from perihelion would account for the band not being seen after June 25th.

Carbon Absorption.

There are a few cases in which we probably have to deal with comparatively feeble manganese absorption, together with the absorption of cool carbon masking the radiation of hot carbon. Here both the hot carbon flutings are affected, instead of one as in the previous cases. With regard to the 564 fluting, we have the cool carbon absorption fluting at 560·7, masking the third maximum of the hot carbon fluting at 554, and the manganese fluting at 558 dimming the first maximum. The result is a band with two maxima as shown in fig. 7, one of these being at 564 and the other at 554 (the

Hot carbon radiation.

Cool carbon absorption.

Manganese absorption.

Integrated result.

Coggia's Comet, 1874.

Fig. 7.—Map showing the result of the integration of hot carbon radiation and the absorption of cool carbon and manganese, compared with Coggia's Comet, 1874.

third maximum of the hot carbon flutings), the latter being the brighter.

With regard to the other hot carbon fluting at 517, we have the cool carbon absorption masking the first maximum, and we get the apparently paradoxical result of the second maximum of the fluting being brighter than the first, as shown in fig. 7.

It is probable, too, that at this stage the outer layers of the hot carbon vapour would also begin to absorb; this would show itself in the brightest least refrangible maxima. Just as the masking of D by the balancing of absorption and radiation gives us the green line of sodium in the absence of D in some of the condensing swarms, we

should here get the second maxima of the two flutings brighter than the first.

This double effect on the carbon flutings at 564 and 517 of masking by cool carbon and manganese was indicated in Coggia's Comet when it was about a month from perihelion, and in the Comet III, 1881, twelve days after perihelion, as shown below :—

Wave-lengths.	Name of comet.	Observer.	Date of observation.	P.P.	P.D.
554—563	Coggia's III, 1874 ..	Vogel*....	June 18 ..	July 8 ..	0·6757
553—563	Comet III, 1881	Copeland†	June 28 ..	June 16 ..	0·7345

Iron Absorption.

In addition to the absorption flutings of lead and manganese as indicated by their masking effects upon the carbon fluting at 564, we have indications of the absorption of the iron fluting at 615.

In Comet Wells Vogel‡ saw on June 2nd (the perihelion passage occurring on June 10th) a bright fluting with its brightest edge at 613, *fading towards the blue, which he attributed to hydrocarbon.* This was undoubtedly a contrast band due to the absorption of the iron fluting at 615. Hasselberg also observed in the same comet on June 5th a fluting with its sharpest edge at 615·7, which he supposed to be the red sodium line at 615. The iron fluting has its maximum at 615, and fades away on the less refrangible side; hence, when absorbing, it will give rise to such an apparent bright band as that observed by Vogel and Hasselberg in Comet Wells.

V. The Final Stage of Heat—Perihelion.

There is evidence to show that when a comet arrives at its shortest distance from the sun, the mean temperature effects are exceeded; and that, speaking generally, a line replaces a fluted spectrum, and we pass from a spectrum very similar to that which we ordinarily get in a glow-tube to one which we cannot produce in it until we employ the highest temperature. The spectral conditions brought about in the comets which in our time have got nearest to the sun, have been almost similar to those observed in the oxy-coal-gas flame; and the recorded observations of the spectrum show that we are dealing with the lines of iron, manganese, and other substances seen at that temperature, which is below that of the electric arc.

We see in the telescope that a comet under the conditions of near

* 'Astr. Nachr.,' vol. 85, p. 12. † 'Copernicus,' vol. 2, p. 225.
‡ 'Astr. Nachr.,' p. 2437.

approach to the sun, puts on the appearance of a central nucleus (or nuclei), with surrounding envelopes, or jets, or both. Because the former now falls upon one part of the slit of the spectroscope, and the latter upon another, the difference between the nucleus and the envelopes is best made out when the comet is nearest to the sun and earth.

When a comet approaches very near to the sun, we get the bright lines, *especially in the spectrum of the nucleus*, so that in addition to the long flutings of carbon (if they be then visible), we have short lines added along the nucleus in the red, yellow, green, and so on.

The lines characteristic of the more volatile substances extend some distance from the nucleus.

It does not always happen, however, that a comet gives a bright line spectrum while near or at perihelion, for the perihelion passage may occur at some distance from the sun, and then the spectrum will be simpler.

In Comets *b*, 1881 (perihelion passage June 16), and *d*, 1882 (perihelion passage September 17), the only lines recorded were magnesium *b*; but the apparent absence of the other lines might be due to continuous spectrum.

It should be noted that the greatest brilliancy and maximum of action is observed *after* perihelion, hence the temperature must be highest after perihelion.

Magnesium Radiation.

In cometary spectra we have already seen that magnesium is first indicated by the fluting at 500, and at a more advanced stage by the fluting at 521. There is evidence to show that magnesium is represented by *b* at perihelion. This was the case in the Great Comet of 1882 as observed by Copeland on September 18th, the day after perihelion passage *b* was probably also seen in Comet III, 1881, by Copeland* (perihelion passage, June 16th). It is described as a well-defined bright line standing at the edge of the bright-green band.

Carbon Radiation.

The disappearance of the flutings of carbon in comets which have short perihelion distances when near perihelion, taken in conjunction with laboratory experiments, at once suggests that the disappearance of the flutings ought to be accompanied by the appearance of carbon lines.

The principal line in the spectrum of carbon is at wave-length 426. This has only been recorded on two occasions, in cometary spectra, namely in Comet Wells. On May 28th (perihelion passage,

* 'Copernicus,' vol. 2, p 229.

June 10th), Copeland recorded a bright line at 426·1, and it was also possibly shown in Huggins's photograph of the spectrum of the same comet taken on May 31st, its wave-length being given as 425·3. On each of these occasions, other evidences of carbon were entirely absent, and the bright lines present in the spectrum gave indications of a relatively high temperature.

There are several reasons why the carbon line spectrum has not been recorded a greater number of times. First, very few comets approach sufficiently near the sun to attain the necessary temperature. Second, the principal line is in a part of the spectrum which is very difficult to observe. Even in the Great Comet of 1882, which was very bright, the observations did not go beyond 465.

This conclusion cannot be regarded as final until careful differential observations of nucleus, envelopes, and jets are made. At present the exact part of the comet the spectrum of which is described is generally not stated, and there is evidence that, up to the highest temperature produced by collisions, carbon in some form is liberated from the meteorites composing the cometary swarm.

The Perihelion Conditions of the Great Comet of 1882.

As the perihelion distances are different in different comets, we must expect the effects to be more decided in some cases than others. The most remarkable case since the beginning of spectroscopic inquiry was afforded by the Great Comet of 1882, most admirably observed by Copeland.

It is found that many of the lines which have been observed at perihelion are coincident with lines seen in experiments with meteorites, while the low temperature lines of magnesium are absent. In the Great Comet of 1882, the lines recorded were the D lines of sodium, the low temperature iron lines at 5268, 5327, 5371, 5790, and 6024, the line seen in the manganese spectrum at the temperature of the bunsen burner at 5395, and a line near *b* which might be due to magnesium, or to a remnant of the carbon fluting. There were also four other lines less refrangible than D, the origin of which has not yet been determined.

The following is a complete list of the lines recorded by Copeland and Lohse* on the day after perihelion passage. The origins of the lines which my observations have suggested are also given.

* 'Copernicus,' vol. 2, p. 239

	Wave-lengths.	Probable origins.
Bright line	602·8	Fe 602·4.
" "	596·3	
" "	595·3	
" "	593·3	
" "	592·1	
Faint soft brightness	590·0	
Bright D_1	589·8	Na.
Bright D_2	588·9	Na.
Short bright line	579·7	Fe 579·0.
Broad band	560·1	2nd max. of Mn 558 fluting.
" "	557·4	Mn 558.
Bright line	547·4	Pb 546·0.
" "	542·8	
" "	539·5	Mn 540·0.
" "	536·9	Fe 537·0.
" "	532·9	Fe 532·7.
" "	526·9	Fe 526·9.
Bright part	520·7	Mg 521·0.
" "	520·3	
A brightness	517·6	Mg (*b*).
Soft band	511·5	
Bright band	510·5	

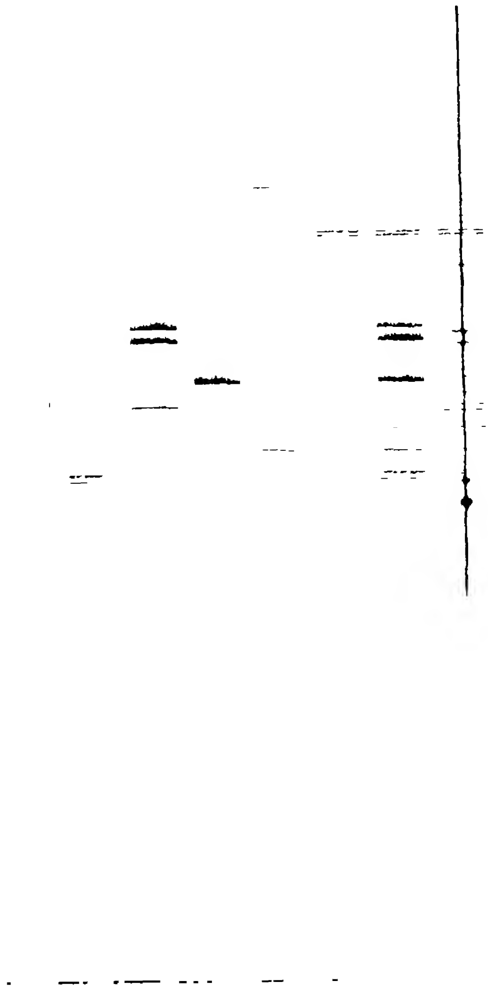


Fig. 8.—Map showing the probable origin of the Spectrum of the Great Comet of 1882 when near Perihelion.

Fig. 8 shows the probable origins of some of the lines in the spectrum of the Great Comet of 1882. The horizontal line which runs through the spectrum represents continuous spectrum due to the bright nucleus.

The Perihelion Conditions of Comet Wells.

Again, in Comet Wells almost the same phenomena were exhibited as in the Great Comet of 1882. In this case the perihelion passage occurred under such conditions that the spectrum of the comet could not be satisfactorily observed on account of the interference of daylight. Detailed observations, however, were made when the comet was near perihelion and its temperature sufficiently high to give bright lines. The following table gives the bright lines and bands with their probable origins, observed in the comet on May 31st, 1882, by Copeland* (perihelion passage June 10th).

	Wave-length.	Probable origins.
A brightness	638·2	
Bright line or nearly so	625·5	
Bright part, line ?	613·3	
" "	598·8	
Bright D_1	589·3	Na.
Bright D_2	588·8	Na.
Sharp bright part	580·3	Fe 579.
Slightly brighter than neighbourhood	573·8	
A bright part, maximum	540·6	Mn 540.
Brightest part in green	512·7	C 513.
Another maximum	501·7	Mg 500.

No origin can at present be suggested for the brightness at 573·8. Copeland only observed it on May 31st, and then noted it as being but "slightly brighter than neighbourhood."

Fig. 9 shows how the spectrum of Comet Wells, on May 28th, can be very closely imitated by integrating the lines and flutings in the above table.

Fig. 10 shows a similar comparison for May 31st, when the comet was a little hotter. In both cases the low temperature fluting of magnesium was recorded; it probably had its origin in some cool part of the comet which was projected on the slit at the same time as the nucleus.

* 'Copernicus,' vol. 2, p. 229.

FIG. 9.—Map showing the probable origin of the Spectrum of Wells' Comet on May 28th, 1882 (P.P. June 10th).

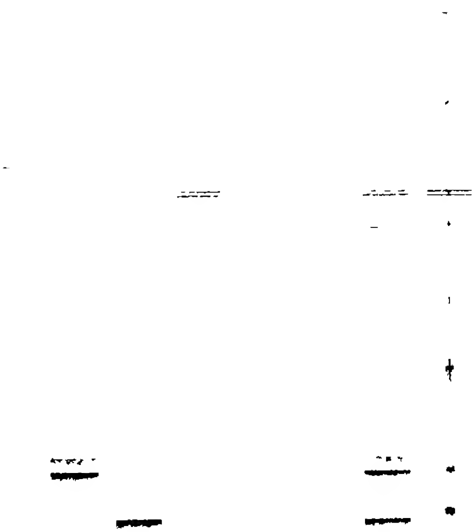


FIG. 10.—Map showing the probable origin of the Spectrum of Wells' Comet on May 31st, 1882 (P.P. June 10th).

Line Absorption at Perihelion.

It has been seen that the first evidence of the appearance of absorption in comets is that afforded by the flutings of manganese and lead, which mask the citron band of carbon. The next indication of absorption is that of the iron fluting at 615.

Line absorption was observed in Coggia's Comet (1874) by Christie, on July 14th, but he gives no definite wave-lengths for the lines seen. He says:—The spectrum of the nucleus was continuous; it appeared to have traces of numerous bright bands, and three or four dark lines also were seen on several occasions, but owing to passing clouds, they were lost before their position could be determined. One appeared to be between D and E, another on the blue side of *b*, and a third near F.*

The perihelion passage of the comet occurred on July 8th.

There were also evidences of absorption in Comet Wells, as observed at Greenwich.

"Two dark spaces were seen near F; the less refrangible one was measured and its wave-length determined as 4862 tenth-metres. It therefore probably is the F line."†

Polariscopic observations have shown that part of the light received from comets is reflected light, and it has been assumed that it is reflected sunlight that is in question. Dr. Huggins, in his valuable memoir on the Comet *b*, 1881 ('Roy. Soc. Proc.,' vol. 33, p. 1), gives a photograph showing absorption lines which he states to be the reflected lines of Fraunhofer. I have not had an opportunity of seeing the original photograph, and it is therefore impossible to speak with confidence, but if the drawing is exact we are not dealing with reflected sunlight, for the hydrogen lines are too strong and the relative thicknesses of H and K are dissimilar. But variations from the solar spectrum are to be noticed in the spectrum of *a* Cygni, and they should be reproduced in a cometary swarm when near the sun.

An additional argument for this conclusion with respect to Huggins's photograph is the absence of ultra-violet continuous spectrum. As shown in the lithograph, the continuous spectrum appears to end rather abruptly, just in front of the group of bright flutings 3883. If we had to deal with reflected sunlight this could not possibly happen.

In describing the spectrum of the Great Comet of 1882,‡ as seen on the morning of September 18th, the day after the perihelion passage, Copeland refers to dark lines which he supposes to be the ordinary

* 'Greenwich Spectroscopic Observations,' 1875, p. 121.

† 'Monthly Notices,' vol. 42, p. 410.

‡ 'Copernicus,' vol. 2, p. 238

Fraunhofer lines. Some of the bright lines observed are described as being to the redward side of dark lines. These are—

D_1,
D_2,
547·4,
542·8,
539·5,
536·9,
532·9,
526·9 (E),
517·6.

In the green there were two bands, one at 560·1, and the other at 557, both as broad as the interval of D, which had sharp dark line on their redward sides.

In all probability these two bands were the first two maxima of the manganese fluting at 558.

The dark lines which Copeland saw were no doubt partly due to the spectrum of daylight, but some were also due to the absorption taking place in the comet itself. The evidence for this conclusion is that some of the dark lines recorded in the cometary spectrum are altogether absent, or are exceedingly faint in the solar spectrum.

Thus there are no dark lines in the solar spectrum to correspond with the dark lines in the spectrum of the comet at 547·4, 539·5, and 517·6. The lines in the spectrum of the comet at 526·9 (E) 532·9, 536·9, 542·8, D_1, and D_2, which also occurs in the solar spectrum, are probably common to both the spectrum of the comet and the daylight spectrum. These are lines which would be likely to appear in the absorption spectrum of the comet, and hence it is highly probable that Copeland observed an integration of the radiation and absorption spectra of the comet and that of daylight.

A comet gives bright lines at perihelion because there is an action which drives the vapours away from the meteorites.

The vapours being driven away with great velocity, the lines in their spectra are displaced if the resolved part of the velocity in the line of sight be sufficiently great. The vapours, however, would surround the meteorites at the moment they were produced by the heat due to impacts, and there would therefore be dark absorption lines which would not suffer displacement. The total result would accordingly be bright lines and flutings corresponding to them arranged alongside each other. This, no doubt, was what Copeland observed in the Great Comet of 1882, the vapours of sodium, iron, and lead were being driven away from the earth, the dark lines being on the more refrangible sides of the bright lines, while the manganese vapours were driven towards the earth, the dark flutings being

consequently (most probably in a different part of the comet) on the redward sides of the bright ones.

VI. General Statement with regard to Carbon.

The earliest spectroscopic observations of comets showed that carbon was a very important element in cometary spectra. Since then, as we have seen, carbon has also been recorded in almost every comet which has been observed, although the spectrum is often greatly modified by the presence of other substances. The experiments on the spectrum of carbon which I commenced many years ago, but which have been temporarily discontinued, show that there are several distinct stages in the spectrum of carbon. At very low temperatures all compounds of carbon give a spectrum consisting of what I have already referred to as the cool carbon flutings. A higher temperature gives what I have called the hot carbon flutings, or carbon A. Finally we get the line spectrum of carbon. Another condition, which is not yet completely understood, is marked by the appearance of the group beginning at 460, which I have called carbon B.* Associated with this are the groups beginning at 420 and 388, the relations of which to the line spectrum I have already discussed in a communication to the Royal Society;† I here reproduce a diagram, fig. 11, which I then gave, showing this relation.

Fig. 11.—Diagram showing the relation to temperature of the carbon line and the violet and ultra-violet carbon B groups. The top horizon indicates the highest temperature.

In the majority of cases the spectrum of a comet has not been recorded until it has arrived at the hot carbon condition, but in the

* Bakerian Lecture, p. 57.

† 'Roy. Soc. Proc.,' vol. 30, p. 461.

case of Winnecke's and Brorsen's Comet, to which reference has already been made, we have evidence to show that this spectrum appeared as the cool carbon spectrum disappeared.

In Winnecke's Comet (perihelion passage June 25th) Wolf's observations on the 17th of June showed the cool carbon spectrum, as I have already stated.

On the 22nd of June Huggins* recorded three bands at wave-lengths 469, 517, and 559. Nothing was recorded near 483, the position of the characteristic cool carbon band, so that we are justified in assuming that the low-temperature condition had changed. The 517 fluting agrees almost perfectly with the principal hot carbon fluting at 516·4. We have seen that the variability of the citron band is one of the principal features of cometary spectra, so that the apparent discrepancy in its position is of no importance here.

The band at 469 was in all probability the hot carbon band which begins at wave-length 474, but has its maximum of brightness at about 468. It is very probable, therefore, that during the time which elapsed between the observations of Wolf and Huggins the spectrum of the comet had changed from that of cool carbon to that of hot carbon. This change is precisely what we should expect, Huggins's observation being the one nearest to perihelion, when the comet was hottest.

Again, we have evidence of the change from the spectrum of cool carbon to that of hot carbon in Brorsen's Comet (1879), the perihelion passage of which occurred on the 30th of March. Konkoly's observation on the 25th of March showed the characteristic cool carbon fluting at 483. Later observations were made by Bredichin† on the 28th, 29th, and 31st March and April 2nd. Eight observations of the citron band gave the wave-length as 551·3. Three measurements of the principal green band gave 510·2 as the mean wave-length, and three of the blue band gave 465·5 as its wave-length. Obviously, there was no cool carbon in the comet spectrum on any of these dates, which are all nearer the date of perihelion passage than the date of Konkoly's observations. It may be remarked that if the blue band is corrected as we have to correct the first green one to obtain the true wave-length (516·4), we obtain a wave-length not far removed from that of the hot carbon band, 474. The apparent displacement of the citron carbon band has before been referred to. As in the case of Winnecke's Comet then, as Brorsen's Comet (1879) approached perihelion, its spectrum changed from that of cool carbon to that of hot carbon.

In Wolls's Comet, as already stated, there was, in all probability, the line spectrum of carbon. All the detailed spectroscopic observations

* 'Phil. Trans.,' vol. 158, p. 556.

† 'Astr. Nachr.,' No. 2257.

of this comet were made between May 20th and June 11th, the perihelion passage occurring on June 10th.

The comet gave indications of a comparatively high temperature during all of this interval, so that the derivation of the line from the fluted spectrum of carbon, or *vice versâ*, cannot be traced.

In addition to this evidence of the existence of carbon in comets, we have further evidence afforded by Dr. Huggins's photograph of the spectrum of Comet III, 1881,* taken on June 24th, the perihelion passage occurring on June 16th. Besides the dark line spectrum to which I have previously referred, the photograph shows three groups of apparent bright lines. Measurements of the two strongest lines in the most refrangible group gave, according to Dr. Huggins, 3883 and 3870 as the wave-lengths. Dr. Huggins says (p. 2):—"The less refrangible line is much stronger, and a faint luminosity can be traced from it to a little beyond the second line at 3870. There can be, therefore, no doubt that these lines represent the brighest end of the ultra-violet group which appears under certain conditions in the spectra of the compounds of carbon. Professors Liveing and Dewar have found for the strong line at the beginning of this group the wave-length 3882·7, and for the second line 3870·5.

"I am also able to see upon the continuous solar spectrum, a distinct impression of the group of lines between G and *h* which is usually associated with the group described above. My measures for the less refrangible group give a wave-length of 4230, which agrees as well as can be expected with Professors Liveing and Dewar's measures 4220."

In addition to the two groups of bright lines above mentioned, a third and fainter group between *h* and H is shown by Dr. Huggins. On the lithograph which accompanies the paper these lines are shown at approximate wave-lengths of 4059, 4052, 4044, and 4038, but no origin is suggested for them.

Messrs. Liveing and Dewar have attributed the two groups first mentioned to cyanogen; but my own researches, which are still far from complete, have not convinced me that this view is correct. I may state, and here Messrs. Liveing and Dewar's observations agree with my own, that the most characteristic cyanogen group is one beginning at about 461; and since there is no trace of this in the photograph, it does not seem likely that the groups seen can be taken as proving the existence of cyanogen.

In a paper which I communicated to the Royal Society in 1880† I described the two groups of lines, or rather flutings, which are referred to in Dr. Huggins's paper, and I also gave their wave-lengths. I have since found that under certain conditions other compounds of

* 'Roy. Soc. Proc.,' vol. 33, p. 2.

† 'Roy. Soc. Proc.,' vol. 30, p. 461.

carbon give the second and last members of the ultra-violet group, at wave-lengths 3873 and 3850, or lines coincident with them, when the other three are entirely absent. I have, however, found no condition under which the first two members of the group, at wave-lengths 3883 and 3870, are as much brighter than the remaining ones, as they are shown in the lithograph which accompanies Dr. Huggins's paper. As shown in the lithograph, the distance between the two brightest members of the group is considerably greater than the distance between the first two members of the ultra-violet carbon group, and if this fairly represents the photograph, the suggestion is that we have to deal with the two lines at 3850 and 3873 to which I have referred. Under the conditions at which these are produced, however, I have never obtained at the same time the group in the blue beginning at 4215, and we should therefore not expect to find them associated with each other in comets. It is also worth noting that nearly all the lines of this group approximate very closely to lines in the flame spectrum of iron. We know that bright iron lines do occur in comets, as, for instance, in Comet Wells and the Great Comet of 1882, and it is nearly certain that the four faint lines between *h* and H are flame lines of iron and manganese; it is quite possible, therefore, that the blue-group is not due to carbon at all. The group of four faint lines is certainly not due to carbon under conditions which we are able to reproduce.

VII. Sequence of Phenomena in Cometary Spectra.

The first stage in the spectrum of a comet is, we have seen, that in which there is only the radiation of the magnesium. The next is that in which Mg 500 is replaced wholly or partially by the spectrum of cool carbon. Mg 5201 is then added, and cool carbon is replaced by hot carbon. The radiation of manganese 558 and sometimes lead 546, is then added. Absorption phenomena next appears, manganese 558 and lead 546 being indicated by their masking effect upon the citron band of carbon. The absorption band of iron is also sometimes present at this stage. At this stage also the group of carbon flutings which I have called carbon B* probably also makes its appearance. As the temperature increases still further, magnesium is represented by *b*, and lines of iron appear. This takes place when the comet is at or near perihelion. At this stage the repellant action of the sun upon the comet is most effective, and if the vapours are driven off in the line of sight with sufficient velocity, the bright lines will suffer displacement. A double set of phenomena would thus be presented; there would be radiation lines of one wave-length from

* Bakerian Lecture, p. 53.

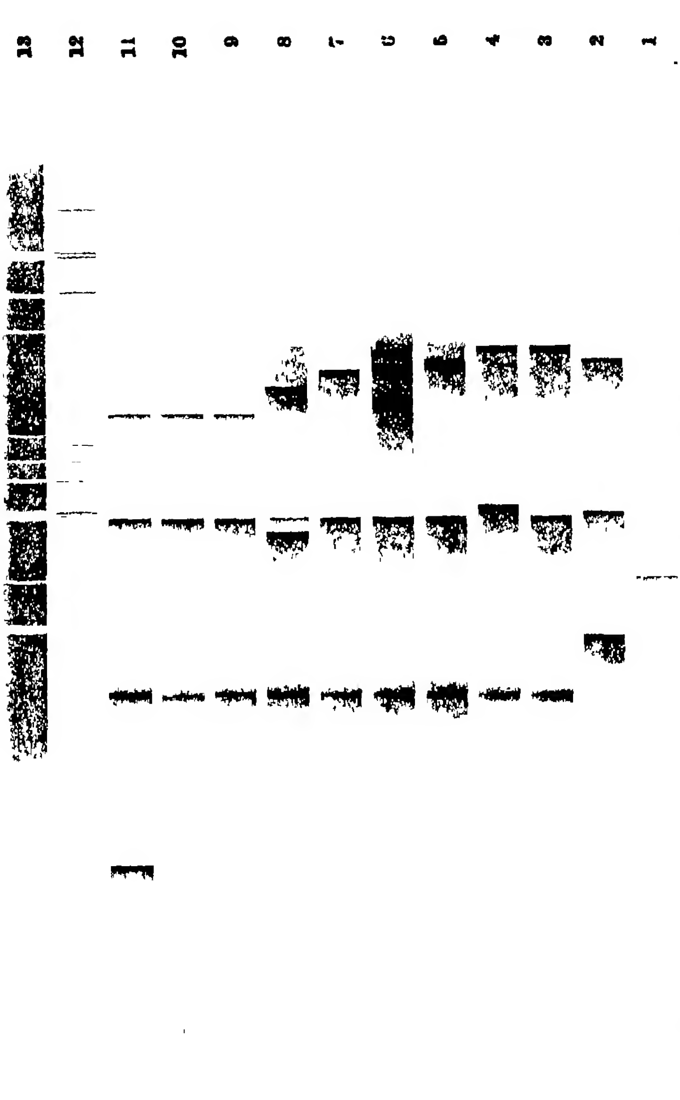


Fig. 12.—Diagram showing the sequence of phenomena in the Spectrum of a Comet. The spectrum at the lowest temperature is shown on the lowest horizon.

Wave-length		Name of Comet.		P.P.	P.D.	Obs	
	Carbon radiation	ke' V, 187	8 Nov., '71	8 Dec. '71		Huggins..	Roy. Soc. Proc.,' vol. 20, p. 45.
	Manganese radiation	,, ,,	11 Nov., '71	,,		Vogel....	Bothk. Beob.,' vol. 1, p. 60.
	Manganese absorption	,, ,,	1 Dec., '71..	,,		Young ..	Amer. Journ.,' vol. 3, p. 81.
	Manganese radiation	ne III, 188	27 July, '81	6 Jun		Copeland	Copernicus vol. 2, p. 223.
	Mn and cool carbon absorption	,,	23 June, '81				,,
	Carbon radiation	G	Nov. 1–18..		'75		Astr. Nachr.,' No. 2716.
55	Manganese radiation		Oct. 22 and 2				Copernicus,' vol. 2, p. 241.

the vapours thus driven off, and absorption lines of a different wave-length from the vapours surrounding the stones in the head.

As the comet recedes from perihelion, these changes take place in inverse order.

The map, fig. 12, represents the sequence which the discussion has shown to be the most probable.

The following is a list of the comets which most nearly approach the conditions represented, the numbers referring to those placed opposite the various horizons in the map :—

13. Great Comet, 1882	Copeland.
12. " " "	"
11. Comet *b*, 1881.............	Huggins.
10. " I, 1882	Vogel.
9. " I, 1868	Huggins.
8. Coggia's Comet, 1874......	Vogel.
7. Comet III, 1868	Huggins.
6. " III, 1881	Copeland.
5. Great Comet, 1882	"
4. Comet *d*, 1880.............	Christie.
3. " III, 1881......... .	Copeland.
2. Winnecke's Comet, 1868 ...	Wolf.
1. Comet I, 1866	Huggins.

This complete sequence has never been observed in any single comet, but it has been continued in some comets where it has been left off in others. Many comets have never been observed beyond the hot carbon stage, whilst others, like Wells's Comet, have not been observed below that stage. Again, this sequence is what we should expect from laboratory observations. The table on p. 191 shows the sequence of the different spectra in a few cases, and it will be seen that in each case, as far as the observations go, the different bands appear in the foregoing order.

In the case of Encke's Comet, 1871 (p.p. December 28th), as the comet approached perihelion, hot carbon radiation was succeeded by the integrated radiations of hot carbon and manganese, and this again by the integration of hot carbon radiation and manganese absorption as shown in fig. 13.

The slight variations shown in the positions of the green band (517) are assumed to be due to errors of observations. As I have already explained, the apparent position of the blue band depends upon temperature, the point of maximum luminosity varying between 468 and 474.

The case of Comet III, 1881 (fig. 14), is a little more complicated, but the general result is the same, namely, that radiation phenomena succeed absorption as the comet recedes from perihelion. Twelve

Fig. 13.—Encke's Comet (P.P., Dec. 28th, 1871).

Comet III, 1881 (P.P., June 16th).

Fig. 14.—Diagram showing the Spectrum of Comet III, 1881, on June 28th and July 27th, showing that absorption occurs nearer to perihelion than radiation.

days after perihelion passage, the spectrum of the comet consisted of the integrated spectra of hot carbon radiation, and the absorption of cool carbon and manganese, as indicated by the masking of the second and dimming of the first maximum of the citron fluting (see fig. 7). A month later still, the absorption bands disappeared, and the spectrum of the comet consisted of the integration of hot carbon, manganese, and lead radiations. On both occasions the blue band had a maximum at 468.

In the Great Comet of 1882 we have a good example of the passage of the spectrum from that of manganese and hot carbon radiations to that of hot carbon alone as the comet cooled. The spectrum recorded by Copeland on October 22nd showed the first condition,

Great Comet of 1882 (P.P., Sept. 17th).

Fig. 15.—Diagram showing the Spectrum of the Great Comet of 1882 at different dates.

and the observations of Gothard between November 1st and November 18th showed the second (see fig. 15).

This sequence may not have been apparent in some comets for two reasons. In the first place, a complication is introduced by the unequal displacements of the bands at different times due to motion in the line of sight, which is variable, and is sometimes very great. Many, apparently, faulty observations are probably to be accounted for in this way.

Again, different observers may not have recorded the spectrum of exactly the same part of the comet, though in general it may be assumed that the brightest part will have been examined. There must be regions of different temperatures in the same comet, and, from what I have shown in this paper, the spectra of different portions will vary considerably. One part of the comet may give hot carbon, whilst another may give cool carbon radiation. The wave-lengths of the bands seen in the two cases would differ, and the results would apparently disagree. In future observations, therefore, it is very important that the exact portion of the comet examined should be stated.

VIII. More Detailed Discussion of certain Comets, with Special Reference to Approach and Recession from Perihelion.

Comet Wells.

Comet Wells was first seen on the 17th of March, 1882, its perihelion passage occurring on June 10th. During the earlier observations, made by Vogel, Tacchini, and others in April, its spectrum presented no feature of special interest, consisting merely of "faint traces of the customary three bands close to the weak, faint, continuous spectrum of the nucleus."* At Greenwich, on May 20th, Maunder suspected "a dark band near D on the blue side of that line," due most probably to the absorption of the second manganese fluting at 586, the first being masked by the citron carbon band.

By May 22nd, when the spectrum was again observed by Vogel, the comet had much increased in brightness, and "the continuous spectrum of the nucleus had increased in intensity and extent, and was not different from the spectrum of a fixed star."

On May 27th, however, Copeland and Lohse noticed a bright line, so faint as to require some attention to see it, in the less refrangible end of the spectrum, which they identified with the D line by comparison on the following day. At the same time they observed a bright part at wave-length 558, due, there can be little doubt, to the first manganese fluting at 558. A maximum at 508 may have been

* Hasselberg, 'Astr. Nachr.,' No. 2441.

due to the low-temperature magnesium fluting at 500. On the 29th of May the spectrum of the comet was again observed by Copeland and Lohse, and the identity of the bright line in the yellow with the D line placed beyond doubt. On the preceding day a Dun Echt circular had announced the discovery as follows:—"The spectrum of the nucleus of Comet Wells deserves the closest attention, as it shows a sharp bright line coincident with D, as well as strong traces of other bright lines, resembling in appearance those seen in γ Cassiopeiæ and allied stars."

Dr. Huggins succeeded on the 31st of May in photographing the spectrum of this comet, and, as was to be expected, could detect no trace of the ultra-violet carbon fluting which was seen in his photograph of Comet *b*, 1881. I have already had occasion to refer to this photographed spectrum.*

On the same day the spectrum of this comet was observed by Maunder, Copeland, Vogel, and others. The most complete record is that made by Copeland and Lohse. They observed "a bright part; line (?)" at wave-length 614·1, for which the reading on the following day gave 615·7. There can be little doubt that this was a contrast band due to the absorption of the low-temperature iron fluting at 615. At the same time there was a maximum brightness in the green at wave-length 501·7, caused most probably by the radiation of the magnesium fluting at 500, in addition to the continuous spectrum.

"A bright part, a maximum" of which the wave-length recorded on May 31st was 543·6, and on the following day 546·8, was due in all probability to absorption by the lead fluting at 546, as I have already explained. It was on this night (May 31st) that Vogel first observed and identified the bright sodium line. "When I examined the spectrum, on May 31st," he writes, "I was greatly surprised by a line in the yellow of great intensity. Measurements and comparisons seemed to identify this line with the sodium line. Yesterday, June 1st, several measurements were made by Dr. Müller, Kempf, and myself, which showed an agreement of the bright line in the spectrum of the comet's nucleus with the D lines; considering the dispersion used this agreement must be called an absolute one. The continuous spectrum extended from about C to deep in the violet. Besides the bright yellow line traces of bright bands were present, perhaps also some dark absorption-lines."† Writing later, he describes the observations of June 2nd thus: "The bright line was, not only in the spectrum of the nucleus, but also in the parts of the comet near to the nucleus, distinctly visible. Besides this, several more bright bands could be seen, which stood out more distinctly when the slit of

* 'Roy. Soc. Proc.,' vol. 43, p. 130.

† 'Astr. Nachr.,' No. 2434.

the spectroscope was not directed on the nucleus itself, but on parts of the comet close to it." He further states that he observed a bright band fading towards the blue, to which reference has been made above, and for which he obtained the wave-length 613. This we have seen was probably a contrast band due to the dark iron fluting at 615. From this date until the comet was lost to view no further change of note took place in the spectrum of this comet.

On June 2nd Vogel observed dark bands in the spectrum of Comet Wells,* but suggests that they might have been due to atmospheric absorption. He says: "The dark absorption-bands, which are still visible in the comet's spectrum, may probably have their origin in our atmosphere, the absorbent action of which, at the inconsiderable height of the comet above the horizon, is very powerful."

Again, Vogel states that dark absorption-bands were possibly present on July 1st, the perihelion passage occurring on the 10th of June. Vogel's suggestion is very important, but since no wave-lengths were determined, it is not possible to say how far it is supported by the facts.

It might, on first consideration, be expected that the changes in the spectrum of a comet as it approaches the sun must be perfectly continuous. The spectrum of Comet Wells, however, was a case in which the changes in the spectrum were apparently discontinuous.

On May 30th and 31st, as already stated, dark bands were observed by Mr. Maunder,† which were in all probability due to manganese absorption.

Between these two dates, *i.e.*, on May 28th, Copeland observed a bright part at 558 which was clearly due to manganese radiation. I have already shown that manganese radiation occurs further from perihelion than manganese absorption. The Greenwich observation of absorption on May 20th, whilst radiation occurs on May 28th, nearer to perihelion, is therefore apparently a discontinuity.

I showed in the Bakerian Lecture that variable stars may be explained by considering the meeting of two meteor-swarms and the consequent increase of temperature due to the impacts. Comets, apparently, go through similar changes and suddenly increase in brightness, as I show in another part of the paper. The explanation is probably the same for comets as for stars, and Comet Wells affords a good example of the fact. It is most probable that on May 20th the comet met another meteor-swarm in its orbit, and an increase of temperature took place; this meant manganese absorption, and this was what was observed.

All the other changes in the spectrum were perfectly continuous as

* 'Astr. Nachr.,' No. 2437.

† 'Greenwich Observations,' 1882, p. 34.

the comet approached the sun, the perihelion passage occurring on June 10th.

The perihelion passage occurred under such conditions that the spectrum of the comet could not be satisfactorily observed on account of the interference of daylight. Detailed observations, however, were made when the comet was near perihelion and its temperature sufficiently high to give bright lines.

I have already discussed the spectrum of this comet when the lines were best seen (May 31st), and the discussion shows that we had remnants of the fluting of magnesium at 500, and of the blue carbon band at 468. The line of carbon at 426 was probably also visible, and the temperature was high enough for the appearance of iron.

As the comet approached perihelion the conditions of observation became less favourable. Between June 5th and June 11th, the perihelion passage occurring on June 10th, nothing but the D lines were recorded. After June 11th the comet was lost.

The Great Comet of 1882.

The spectrum of the Great Comet of 1882 was first observed on September 18, a day after perihelion, by Copeland.*

The spectrum consisted of bright and dark lines, among which was the bright yellow line of sodium, several bright lines in the green, E, and some prominent iron lines and five well-defined bright lines on the red side of D. These have already been referred to. In addition there were two dark lines on the redward side of 558 and 560, which were most likely the edges of the first two maxima of the manganese absorption fluting at 558. No more observations could be made at Dun Echt until September 29, and in the interval most of the bright lines in the spectrum had disappeared, whilst the carbon bands had made their appearance. The D lines were still bright, but E and the other lines had vanished. There was, however, something which is described as "almost a line" at 610·3; this, no doubt, was the iron fluting at 615.

The next observations of the comet were made by Vogel,† on the 1st, 5th, 6th, and 7th October. On each of these occasions D was still visible as a bright double line, in addition to the ordinary cometary flutings.

When the next observation was made on October 16th, by Hasselberg,‡ D had disappeared. On the 22nd and 23rd October, Copeland again observed the spectrum, and it then consisted of the three ordinary cometary bands; the citron band had a maximum at about wave-length 557. Here Mn radiation had evidently commenced.

* 'Copernicus,' vol. 2, p. 237.

† 'Astr. Nachr.,' No. 2466.

‡ 'Astr. Nachr.,' No. 2473.

The later observations of Gothard* and Konkoly,† showed nothing but the three ordinary bands.

No observations were made after the comet had got sufficiently cool to show either the cool carbon flutings or the magnesium fluting at 500.

Although the observations are not perfectly continuous, there is conclusive evidence that the reduction in temperature of the comet consequent on its departure from the neighbourhood of the sun was accompanied by the following changes in its spectrum:—

18th September. Bright and dark iron lines and manganese flutings.

29th September. Bright flutings of iron.

22nd October. Bright manganese.

1st November. Hot carbon radiation.

The two latter stages have already been specially referred to (p. 193).

No doubt if further observations had been possible the flutings of hot carbon would have been replaced by cool carbon flutings, and these again by magnesium 500.

Coggia's Comet.

The perihelion passage of this comet occurred on July 8th, 1874, and the available observations of its spectrum date from May 18th to July 14th. On May 18th, Vogel‡ observed three bands, one of which was at wave-length 515. This was probably the hot carbon fluting at 517, but as the wave-lengths of the other bands are not given, it is not possible to come to a definite conclusion.

On the 18th May Vogel again recorded the three bands, the principal one commencing at 516·5, and having a second maximum at 512. It is probable that these were the first two maxima of the green carbon band, the wave-lengths of which are about 517 and 513.

On June 4th, the date of Vogel's next observation, the three bands were still visible. The wave-lengths are given as 562, 514, and 473.

On June 7th, Vogel's observation recording three bands at 557, 518, and 473, give evidence of manganese absorption, as indicated by the apparent displacement of the citron carbon band in the manner I have already explained.

On June 13th, 14th, and 15th, in addition to the absorption of manganese, there was probably the absorption of cool carbon, as indicated by the masking of the 2nd maximum of the citron carbon band, as I have already explained.

D'Arrest's observations§ on June 15th, 16th, and 17th, show that

* 'Astr. Nachr.,' No. 2472 and 2716.

† 'Astr. Nachr.,' No. 2475.

‡ 'Astr. Nachr.,' No. 2018.

§ 'Astr. Nachr.,' No. 2001.

the manganese absorption was increasing, whilst the carbon was probably beginning to fade out.

The later observations of Vogel, on June 22nd, and of Christie,* between July 3rd and 14th, are incomplete, inasmuch as the positions of all the bands were not determined. Vogel gives the position of the green band as 515, but simply states the presence of the citron and blue band. Christie states that two of the bands were sensibly coincident with the two principal bands in the spectrum of carbon dioxide (probably carbon 517 and 474), but the position of the third band was not determined. It is scarcely possible, therefore, to say how far the indications of manganese absorption have increased between June 22nd and July 14th. Christie states, however, that there was line absorption on July 14th, six days after perihelion. I have stated in another part of the paper that the highest temperature effects do not occur until the comet is some distance beyond perihelion, and this is a case in point.

As Coggia's Comet approached perihelion, therefore, after having first become visible, the first recorded change in its spectrum was the addition of manganese absorption to carbon radiation, but the discussion of other cometary spectra shows that there was probably an intermediate stage between June 4th and June 7th, when instead of manganese absorption, manganese radiation was added. A little later cool carbon absorption was added. Finally, just after perihelion, fluting was replaced by line absorption.

In observations in my own observatory with my 6½-inch refractor, I obtained indications that the blue rays were singularly deficient in the continuous spectrum of the nucleus of the comet; and in a communication to 'Nature'† I suggested that this fact would appear to indicate a low temperature.

This conclusion was strengthened by observations which I made at Newcastle with Mr. Newall's telescope. The colour, both of the nucleus and of the head of the comet, as observed in the telescope, was of a distinct orange yellow, and this, of course, lends confirmation to the view expressed above. While ten minutes' exposure of a photographic plate gave no images of the comet, the faintest of seven stars in the Great Bear gave an impression in two minutes.

The fan also gave a continuous spectrum but little inferior in brilliancy to that of the nucleus itself; while over these, and even the dark space behind the nucleus, was to be seen the spectrum of bands, which indicates the presence of a rare vapour of some kind, while the continuous spectrum of the nucleus and fan, less precise in its indications, may be referred either to the presence of denser vapour or solid particles.

* 'Greenwich Observations,' 1875, p. 121.
† 'Nature,' vol. 10, p. 180, 1874.

I found that the mixture of continuous band spectrum in different parts was very unequal, and, further, that the apparently continuous spectrum changed its character and position of maximum. Over some regions it was limited almost to the region between the less refrangible bands.

I wrote at the time:—

"It is more than possible, I think, that the cometary spectrum, therefore, is not so simple as it has been supposed to be, and that the evidence in favour of mixed vapours is not to be neglected."

Comet III, 1881.

The perihelion passage of this comet occurred on June 16th. I have already remarked that Copeland* observed on June 25th a dark band at 567·9 in this Comet, in addition to the hot carbon radiation. This band was probably due to lead at 568, the first band at 546 being masked by the hot carbon. Manganese absorption was also indicated on the same date. On June 25th the spectrum of this comet was photographed by Huggins, and the carbon B group of flutings was stated to have been seen, giving indications of a relatively high temperature. As the comet receded from the sun other phenomena were observed. On June 27 magnesium at 520 was detected by Hasselberg; manganese absorption was again indicated in Copeland's observations on June 28, and manganese radiation on June 29 and July 27. I have already had occasion to refer to these two conditions (p. 193).

No observations were made on the comet after July 27, or the hot and cool carbon flutings would doubtless have been recorded alone. Carbon radiation is indicated in all the observations that were made from June 25 to July 27.

It should also be noted that hydrocarbon at 431 was observed on June 28th, by Copeland; but neither before nor after this date was hydrocarbon recorded. The reason probably is that the band is too far in the violet to be very manifest. Copeland recorded it as "a bright line, common to spirit-lamp and comet," and hence there can be no mistake as to its identity.

Brorsen's Comet.

The observations of this comet at its appearance in 1868, made by Secchi† between the 23rd and 27th of April, 1868, and by Huggins‡ between April 29th and 13th May, 1868, perihelion passage occurring on April 20, 1868, differ very considerably.

Secchi observed flutings at 473, 512, and 553. The first of these

* 'Copernicus,' vol. 2, p. 225.

† 'Comptes Rendus,' vol. 66, p. 882.

‡ 'Roy. Soc. Proc.,' vol. 16, p. 386.

agrees almost exactly with the blue band of hot carbon, and if the two other bands be shifted by equal amounts, so that the first one coincides with hot carbon 517, and the second consequently with manganese 558, we have indications of manganese added to carbon radiation; the description of the band, however, is insufficient to enable us to say whether the manganese was radiating or absorbing.

Huggins gives flutings at positions which, when reduced, give 464, 508, and 544, as the wave-lengths. The wave-lengths of the two less refrangible ones are apparently shortened, as if they were shifted towards the blue. It is probable, however, that manganese was indicated by the observations of Huggins, for if we shift the band at 508 to 517, the 544 band becomes 553, which is not far removed from the manganese fluting. The drawing given by Huggins shows this as a somewhat narrow band, fading away in both directions, which would seem to show that there was manganese radiation added to carbon radiation, as I have previously explained. This being so, since Huggins's observations were made when the comet was further from perihelion than at the time of Secchi's observations, the discussion of the sequence of changes in other cometary spectra suggests that in Secchi's observations we had to deal with the absorption of manganese.

In a note on the spectrum of Brorsen's Comet at the next return (1879), Professor Young* refers to Huggins's observation. He states that "the only special interest in this (Professor Young's) observation lies in the fact that in 1868 Mr. Huggins obtained a somewhat different result for the same comet." He further goes on to say: "I am entirely at a loss to explain Mr. Huggins's result. It can hardly be that the comet has really changed its spectrum in the meanwhile, and a careful reading of his account ('Roy. Soc. Proc.,' vol. 16, p. 388) gives no light as to how an error could have crept into his work; on the other hand, every precaution would seem to have been taken. However this may be, I am quite positive as to the accuracy of my present result—that the middle band of the spectrum of this comet now coincides sensibly (to a one-prism spectroscope) with the green band in the hydrocarbon spectrum."

I have now shown that the spectrum of a comet is by no means a constant, but depends upon the distance of the comet from perihelion passage. The spectrum is, therefore, not necessarily the same at two different returns, as Professor Young supposes, although it may be the same at equal distances from perihelion.

It is impossible, however, to explain Huggins's observation of Brorsen's Comet without assuming a shift, which is probably instrumental. In the face of this difficulty, I venture to suggest the above as the probable explanation of the spectrum of this comet.

* 'Amer. Journ.,' vol. 17, May, 1879.

There are no further observations which might enable us to further trace the sequence of spectroscopical phenomena in the comet at this return.

At the next return, however (perihelion passage March 30, 1879), several observations were made on different dates. Low temperature carbon bands were recorded on 25th March, 1879.* Bredichin† made a series of observations, extending from 26th March to 2nd April, but only gives one set of wave-lengths, as if no change had occurred in the spectrum of the comet during the interval. The observations, however, seem to indicate hot carbon with manganese absorption.

An observation was made two days after perihelion by Young,‡ who observed bands near 476 and 560, and measured one at 512. These are probably hot carbon bands with manganese absorption; in the case of the green band at 512, the first maximum of the fluting at 517 was probably masked in the way I have already explained, so that the second maximum at 513 was the brighter. On April 17, the Astronomer Royal§ observed cool carbon bands in the comet's spectrum.

Messrs. Copeland and Lohse‖ observed the comet from April 16 to May 2, and give 547·6, 515·6, 469·6 as the wave-lengths of three bands. Of the band at 547·6 they say, "it was very ill defined on both sides, and being without any definite brighter part, its wave-length is very uncertain." The measurements made on April 16 are not given separately, nor is it definitely stated that any measurements were made on that day. The apparent discrepancy of hot carbon being seen when the comet was further from perihelion than when cool carbon was seen, is most probably another case of a comet temporarily passing through a meteoric swarm, and thereby increasing in temperature, as was the case with Comet Wells, 1882, on May 20th.

Winnecke's Comet in 1877.

Winnecke's Comet, 1877, was observed by Lord Lindsay¶ on April 18th, a day after perihelion. Its spectrum presented much the same characteristics as in 1868. Bands at 472·2, 516, and another near 556 were observed. The strongest was at 516 and the band at 556 is given as very weak.

We, no doubt, have here another case of manganese absorption occurring in conjunction with hot carbon radiation, when a comet is near perihelion. On May 5th, the spectrum of the comet gave every indication of hot carbon in conjunction with manganese radiation, the

* C. Konkoly, 'Astr. Nachr.,' No. 2269.
† 'Astr. Nachr.,' No. 2257.
‡ 'Amer. Journ.,' vol. 17.
§ 'Monthly Notices,' vol. 39, p. 429.
‖ 'Monthly Notices,' vol. 39, p. 430.
¶ 'Monthly Notices,' vol. 37, p. 430.

band given at wave-length 558 being evidently due to the radiation of the latter element, since the band fades away in both directions.

Another band was measured at 467·9, and is most probably the carbon band at 474 which under certain conditions has it maximum at 468 instead of 474.

On May 6th the comet was again observed. A very faint line was seen at 569 and another at 543. These were probably due to the lead flutings at wave-lengths 568 and 546.

The apparent absence of lead in the spectrum observed on May 5th may probably be due to the incompleteness of the observations on that date in comparison with those made on May 6th. Or it may be that the greater brightness of the continuous spectrum masked the two faint remnants of the lead fluting.

Other bands were observed on May 6th, the hot carbon and the manganese radiation at 558 being clearly indicated.

An observation was made on May 15th at Greenwich* and it is interesting to note the change that had taken place. A band at 517 was measured, and two others observed, one about 483 and another about 561. Here, clearly, we have indications of cool carbon radiation occurring as the comet receded from the sun, the observations having been made nearly a month after perihelion.

As the comet receded from the sun, then, manganese absorption was succeeded by manganese radiation, hot carbon being indicated in both cases. No further observations were made until nine days after the latter condition was observed, and then the spectrum was that of cool carbon. Doubtless there was an *intermediate stage* in which hot carbon was observed alone.

IX. Possible Causes of Collisions in Comets.

Internal Work.

Professor Tait's view as to the origin of collisions in a meteor-swarm entering our system as a comet was that they were a consequence of the movement of the individual meteorites along approximately elliptic orbits, described in something like equal periods in any plane about their common centre of inertia.

The group was also supposed to be subjected to a sort of tidal disturbance by the sun.†

It is certain that one of the principal causes of the increase of temperature of a comet during its approach to perihelion is the increased number of collisions due to the greater tidal action which takes place. Hence the larger the swarm, the greater the difference between the attractions of the sun upon opposite sides of it, and therefore the greater the disturbance set up. Also, the shorter the

* 'Greenwich Observations,' 1877.

† 'Edinb. Roy. Soc. Proc.,' vol. 10, p 367, 1879.

perihelion distance, the greater fraction of it is the diameter of the swarm, and the greater therefore the differential attraction.

The initial movements of the individual members of the swarm, and these superadded by tidal action, may be defined as producing *internal* work.

If all the heat of a comet is produced by such internal work, it is clear that the temperature of the comet will depend (1) upon the velocity of orbital motion of the particles, (2) upon the size of the swarm of which it is composed, and (3) upon its perihelion distance. It will practically be independent of the velocity of the comet in its orbit round the sun.

While some comets at perihelion give such high temperature phenomena as were observed in Comet III, 1881, Wells's Comet, and the Great Comet of 1882, others, like Winnecke's Comet, 1868, give only the spectrum of carbon.

These differences are what we should expect from the known perihelion distances, and it must be understood that the four stages into which the different degrees of activity in a comet have been divided in this paper are those which occur in a comet with a short perihelion distance. In comets with a long one, perihelion effects may only be equivalent to mean distance effects in comets with short perihelion distances.

I have prepared the following list of the perihelion distances of the comets which have been discussed, the distances being given in terms of the astronomical unit, derived from the data given in the 'Annuaire du Bureau des Longitudes.'

In the various tables which precede, for each comet the date of observation, perihelion passage, and perihelion distance are stated.

Name of comet.	Perihelion passage.	P. distance.	Reference.
			'Annuaire Bureau des Long.,'
Comet I, 1864 ...	Aug. 15, 1864	0·90929	1885, p. 199
Brorsen	April 20, 1868	0·596762	1874, p. 100
"	March 30, 1879	0·589892	1883, p. 240
Winnecke	June 26, 1868	0·781538	1874, p. 100
Comet I, 1871..	" 10, 1871	0·6543	1883, p. 210
Tuttle's........	Nov. 30, 1871	1·03011	1883, p. 240
Encke	Dec. 28, 1871	0·332875	1874, p. 100
Comet IV, 1873..	Sept. 10, 1873	0·7940	1883, p. 216
Coggia's, 1874 ...	July 8, 1874	0·6757	1884, p. 262
Comet I, 1874 ...	Aug. 27, 1874	0·9826	1883, p. 221
Comet I, 1877 ...	Jan. 19, 1877	0·8074	1883, p. 222
Winnecke, 1877..	April 17, 1877	0·9499	1883, p. 223
Comet *d*, 1879 ...	Oct. 4, 1879	0·9896	1883, p. 227
Comet III, 1881..	June 16, 1881	0·7345	1884, p. 252
Comet Wells	" 10, 1882	0·06076	1884, p. 258
Gt. Comet, 1882..	Sept. 17, 1882	0·007753	1884, p. 262

External Work.

If external work is done on a comet by meteorites in space, that is to say, if there are collisions with external bodies, the velocity of the comet must be considered in the first place, and the equal or unequal distribution of the masses which it encounters can be tested by the phenomena observed.

The discussion of the recorded observations shows, indeed, that in addition to the constantly increasing action which takes place in a comet during its approach to perihelion passage, there are at times temporary increases in temperature.

We know that meteorites are scattered through space, and here and there are gathered into swarms. It is only to be expected, therefore, that at times a comet will meet with such swarms just as our own planet does, and in that case its temperature would be increased by the collisions which would occur. The increase of temperature would depend upon (1) the dimensions and density of the swarm; and (2) upon its velocity. The larger and denser the swarm the more collisions would be likely to occur, and the greater the velocity of the comet the greater the amount of kinetic energy available for transformation into heat energy.

If the density of the meteoritic plenum increases towards the sun, the external work done will increase with it.

Collisions between Cometary and other Swarms.

We have then not only to consider the increased activity in a comet due to its approach to perihelion, but we have also to take into account the possibility of its passing through other swarms of meteorites during its revolution. That such collisions do take place there can be little doubt. Sawerthal's Comet, 1888, which increased in brightness by three magnitudes in two days, is a case in point.* Unfortunately, no spectroscopic observations were made, or no doubt the effects of the increased temperature upon the spectrum would have been apparent.

The spectroscopic observations of Comet Wells seem to show that this comet also passed through at least one swarm during its revolution. An observation at Greenwich, on May 20th, recorded dark absorption lines, which I have shown to be especial to high temperatures in comets. Between that date and perihelion passage (June 10th) there were evidences of a lower temperature, as I show in another part of the paper. I am not aware of any observations recording an increase in brilliancy of the comet on May 20th, but if they do exist, they will obviously strengthen this view.

Perhaps the case of greatest importance, however, is the Great

* 'Nature,' vol. 38, p. 258.

Comet of 1882. At perihelion, this comet was only 300,000 miles from the photosphere of the sun, and it was practically as bright as the sun itself. Mr. Finlay, at the Cape, followed the comet until it apparently rushed into the sun. That a comet should be able to pass within so short a distance of the sun without suffering entire disruption has been used as an argument against the existence of an extended solar corona. My own view of the case, however, is that the evidence afforded by this comet of the existence of a meteoritic solar atmosphere is most conclusive.

That it would be impossible for a comet to pass through a gaseous atmosphere is proved by our terrestrial experience with falling stars, but if the regions far above the sun's photosphere are constituted as I have suggested,* we should expect a transcendental clashing effect, but no change in the orbits of the meteorites which were not engaged.

I would submit, therefore, that the immediate cause of the enormous increase in brilliancy of the comet, which enabled it to be obtained close to the sun's disk, was undoubtedly the collisions which took place between the meteorites constituting the comet, and those which occupy the outer cooler regions of the sun. Not only does this event demonstrate the existence of an outer solar atmosphere, therefore, but it also points to its meteoric nature, the meteorites there being probably formed by the condensation of metallic and other vapours, exactly in the same way as we have snow and raindrops in our own atmosphere. Observations by Messrs. Finlay and Elkins before and after perihelion showed that the comet was not perceptibly retarded by its adventure, which is quite consistent with my view, collisions between individual meteorites would not retard the motion of the comet as a whole.

Another case of considerable interest is the Pons-Brooks Comet, 1883—1884. At its last return this comet was first observed by Mr. Brooks on September 1, 1883; it passed perihelion on January 25th, and was last seen on June 2nd, 1884. It was distinguished by its sudden fluctuations in brilliancy, which no doubt were caused by its intersection with other swarms. On September 21st, it was observed by Mr. Chandler, at Harvard,† as a faint nebulosity with a slight condensation. On the 22nd, it was represented by an apparent star of the eighth magnitude, according to the observations of Schiaparelli,‡ the luminosity having been augmented eight times within a few hours.

In a short time, the comet again appeared as a nebulous disk. This sudden change has an exact parallel in "new stars," and the cause is

* 'Roy. Soc. Proc.,' vol. 40, p. 357.

† 'Astr. Nachr.,' No. 2553.

‡ 'Astr. Nachr.,' No. 2553.

no doubt the same in both cases. The rapidity with which the comet cooled demonstrates that only small masses could be in question. This took place whilst the comet was no less than 200 million miles from the sun.

On October 15th there was a similar occurrence in the same comet, and again, a more decided one on January 1st. In the latter case, in less than four hours,* the comet had become an apparent star, and again assumed the cometary form.

In these cases, then, we have evidence that the luminosity of the comets depends first upon its distance from the sun, and secondly upon distribution of other swarms along its path.

It would appear that a further discussion from this point of view might afford us interesting information on several points.

X. On some Effects of Collisions in Comets.

If we assume that the increased brightness of comets as the sun is approached depends to any extent on collisions with meteorites external to the swarm, we must conclude that such meteorites exist nearer together nearer the sun. The idea seems strengthened by the great and irregular variations of intensity sometimes observed, as we know that the meteorites which the comet is liable to meet are not equally distributed. Such a variation was noticed in Sawerthal's Comet in 1888, as I have already stated.

Such variations, however, would be more likely to be observed in the tails in consequence of the enormous dimensions of some of them. Such variations have been observed from the time of Kepler.

The fact that these variations so strongly resemble at times auroral displays is an additional argument in favour of the meteoric origin of the latter.

Another result of a different order produced by a comet moving through a meteoric plenum would be the gradual shortening of a comet's periodic time as the result of collisions, and this shortening should not be absolutely regular, as in a homogeneous gas, for the reason that the meteorites are not equally distributed.

That there is such a shortening was proved by Encke for the comet which bears his name, and that there are irregularities the following table will show, though how far they might have been due to perturbations has not, I believe, been so far studied :—

* Dr. Müller, 'Astr. Nachr.,' No. 2568.

Returns of Encke's Comet, showing Reduced Period of Revolution.

	Observed period of revolution.			Difference.	
	days.	hrs.	mins.	hrs.	mins.
From 1786 to 1795, three times	1212	15	7		
„ 1795 „ 1805 „ „	1212	12	0	3	7
„ 1805 „ 1819, four „	1212	0	29	11	31
„ 1819 „ 1822..................	1211	15	50	4	39
„ 1822 „ 1825.......	1211	13	12	2	38
„ 1825 „ 1829.............	1211	10	34	2	38
„ 1829 „ 1832...	1211	7	41	2	53
„ 1832 „ 1835...........	1211	5	17	2	24
„ 1835 „ 1838	1211	2	38	2	39
„ 1838 „ 1842....	1210	23	31	3	7
„ 1842 „ 1845..................	1210	21	7	2	24
„ 1845 „ 1848..................	1210	18	29	2	38
„ 1848 „ 1852..................	1210	17	2	1	27
„ 1852 „ 1855..................	1210	11	17	5	45
„ 1855 „ 1858..................	1210	13	41	21	36

There is still another point. If the luminosity were due entirely to internal collisions brought about by the increase of solar action, then large comets, or those best visible, should begin to be brilliant long before smaller or more distant ones. But this does not seem to be so. Mr. Hind has pointed out that proximity to the earth is not so important a condition for visibility of a comet in the daytime as close approach to the sun*; and M. Faye is the authority for the statement that no comet has been seen beyond the orbit of Jupiter.† "It is assuredly not on account of their smallness that they thus escape our notice in regions where the most distant planets, Saturn, Uranus, and Neptune, shine so clearly with the light which they borrow from the sun; this is because the rare and nebulous matter of comets reflects much less light than the solid and compact surfaces of the planets of which we speak, much less even than the smallest cloud of our atmosphere."

On the latter part of this quotation it may be remarked that it is not necessary to assume that comets at a great distance from the sun, any more than nebulæ, are visible by means of reflected light.

Olbers, Faye, and others have attributed the production of comets' tails to solar repulsion. Away from the sun, as we have seen, comets are tailless.

The tail of a comet usually grows with its approach to the sun. This is not merely an apparent increase due to diminished distance,

* 'Nature,' vol. 10, p. 286.
† 'Nature,' vol. 10, p. 228.

but is a steady growth outwards. The tail of a comet is always directed away from the sun, so that it sweeps round in a semicircle as the comet passes through perihelion. The apparent repulsion of the tails suggested to Olbers in 1812 the idea that the materials composing them are subject to electrical repulsion proceeding from the sun, that they consist, in fact, of small electrified particles repelled by the similarly electrified sun.

As a rule, the tail increases very quickly and considerably in length *after* perihelion passage. Thus Borelly's Comet of 1874 increased from 4° to 43½° in length from July 3rd to July 19th in that year, or from 4 millions to 25 millions of miles in length.* This effect is precisely what we should expect if the tail be fed by vapours due to collisions, for at perihelion the tidal action, and therefore the interior movements, will be greatest; besides which it is probable that collisions with meteorites external to the swarm will here be more frequent and more heat-producing on account of the highest velocity of the comet.

M. Bredichin, of the Moscow Observatory, has shown that there are three distinct types of tails. In the first class, the tails are long and straight, and the repellent energy of the sun upon the small particles is about twelve times as great as the energy of his gravitational attraction. The particles therefore leave the nucleus with a high velocity, generally about 14,000 or 15,000 feet per second. The greater this velocity in relation to the rate of travel of the comet, the straighter of course will be the tail, because the particles forming it do not lag behind. In the second type, the energies of the attraction and repulsion balance each other, or nearly so, and the tails of this class are plumy and gently curved. In this case the particles which go to form the tail leave the head with a velocity of about 3000 feet per second.

Tails of the third type are short and strongly bent, the repellent energy being only about one-fifth of the attractive energy of the sun, and the velocity of the particles leaving the head is only about 1000 feet per second.

Many comets exhibit tails of more than one type, and it was conjectured long ago that such tails were composed of different kinds of matter.

Bredichin went further, and defined the composition of the different kinds of tails which he had classified, by referring to the weights of the materials which would give the relative values of the repulsive and attractive forces necessary for tails of the different types. He thus found that the long straight tails of the first type would be probably formed by hydrogen, since this substance, on account of its exceeding lightness, would be little influenced by gravity, while at

* Hind, 'Nature,' vol. 10, p. 252.

the same time strongly influenced by the electrical repulsion. The second type of tails he considered to be made of hydrocarbons, since hydrocarbons have a specific weight such that the repellent and attractive forces of the sun upon their particles may be nearly equal. Iron, on the other hand, would be more subject to the action of gravity, on account of its greater weight, and was therefore taken as adapted to tails of the third type.

The observations on meteorites recorded in the Bakerian Lecture, and the discussion of cometary observation contained in this Appendix, show that the vapours which are given out by the meteorites as the sun is approached, are in an approximate order:—

Slight hydrogen.
Slight carbon compounds.
Magnesium.
Sodium.
Manganese.
Lead.
Iron.

Now of these the hydrogen and carbon compounds are alone permanent gases, and the idea is that they have been occluded as such by the meteorites. They are given out as the temperature of the meteorite again increases.

Tails extending 10,000,000 miles through the cold of space, cannot, as Brodichin supposes, I suggest, be composed of iron vapour, but they may well be, and doubtless are, of the hydrogen and various carbon compounds.

The magnesium and iron vapours will condense soon after their repulse from the meteorite, the volatilisation of which produced them, and here, as Reichenbach with marvellous prescience suggested in pre-spectroscopic times, we have the chondroi of the exact chemical nature which he postulated.

There is nothing extravagant in these suppositions, for we now know that all the substances in question do exist in comets, and it is evident that much is to be learnt from a continuation of the inquiry.

We know that the short-period comets get less brilliant with every approach to perihelion, and that some do not even throw out a tail, and we can easily ascribe both these results to the fact that after several such appulses the vapours liable to be driven out of the meteorites by temperature get less and less.

If this be so, we may regard the comet with many tails as one which for the first time undergoes perihelion conditions. We are in presence of the "unperihelioned matter" glimpsed by Sir William Herschel.

Further, it is important to associate the spectra of the envelopes and nucleus with the multiplicity of tails.

Let us suppose a comet's tail thus chemically constituted; the molecules will be moving rapidly under the influence of the solar repulsion away from the meteorites which produce them, through a *meteoritic plenum*. Hence we should expect auroral phenomena. These have been recorded in comets' tails since the time of Kepler. In the tail we have gases moving through meteoritic dust, in the aurora, as I shall show in the next part of this memoir, we have in all probability meteoritic dust moving through gases.

What then becomes of the tails?

Being thus formed at the expense of the materials composing the head, the materials removed from the head can never be returned to it because of its insufficient gravitational power over them, and moreover they can no longer traverse the same orbits as the meteorites from which they sprung, because they have already been turned out of that course by the forces attending the development of the tail. The gaseous bodies thus become distributed throughout the space occupied by our system, and give no farther trace of their existence until, after subsequent occlusion which causes their disappearance, they are again made evident by future collisions. The existence of "unperihelioned matter" then indicates that the regions of space nearer the sun are not so full of these free gaseous products as those further away.

Comets must thus degenerate, so far at all events as their easily volatilised constituents are concerned, with each perihelion passage, but as the majority of them only approach the sun at long intervals of time they do not suffer much in this way. Some of the short-period comets get less and less brilliant at each successive perihelion passage, and others are then observed entirely without tails, all the available tail-forming material having been used up and dispersed into the regions of space farther away from the sun, while at aphelion a fresh supply has been lacking.

It has been conjectured by Weiss and Schiaparelli that the condensed metallic materials of the tails, which are projected with the tails in the cases of the comets whose perihelia lie within the earth's orbit, may give rise to the appearance of meteors.

This may also happen in the case of condensable materials shot in the first instance towards the sun, so that we may imagine the original train of meteorites to gradually widen out in the plane of the orbit inside and outside of the orbit of the main swarm.*

It has been suggested that the luminosity of comets is possibly partly electrical, and in support of this view Hasselberg showed that the changes in Wells's Comet were closely related to changes which

* Herschel, 'Monthly Notices,' vol. 35, p. 253.

took place in an electrically illuminated vacuum tube, containing hydrocarbon and sodium.

Before referring to this, however, I may mention an early experiment of my own in connexion with this point.

I described this experiment in the 'Manchester Science Lectures,' 1877 (p. 130), but it was made some years before.

A mixture of meteorites taken at random was placed in a tube attached to another tube with arrangements for passing electric sparks, and this again was connected with a Sprengel pump. After exhaustion, on passing the current under conditions which are generally supposed to give a spark of low temperature, the spectrum was seen to be that which Huggins, Donati, and others had observed in the spectrum of the head of a comet. The gases occluded in meteorites were thus shown to be exactly what we get in the head of a comet.

A Leyden jar was then included in the circuit, and the spectrum of carbon was seen to have been replaced by that of hydrogen, from the decomposition of hydrocarbons. Under low temperature conditions, then, the spectrum was that of carbon, while under high temperature conditions the spectrum was that of hydrogen. I also stated that in my laboratory work I had come across other curious cases in which compound vapours when dissociated only gave us one spectrum at a time, meaning that in a vapour consisting of two well-known substances, under one condition we only get the spectrum of one substance, and under another condition we get the spectrum of the other substance alone, so in others again of both combined.

I had noticed this change very particularly during the researches of Professor Frankland and myself, in 1869, on the spectrum of hydrogen. In this case the two substances to be considered were hydrogen and the mercury vapour from the mercurial air-pump which was employed in the experiments.

In the subliming experiments I also found that a carbonaceous meteorite *in vacuo* gives off hydrocarbon vapour at the ordinary temperature, as a weak electric discharge gives us the longest line in the band spectrum of carbon without heating. On heating, the other lines come in till the well-known bands are formed with more or less completeness. If the discharge be a little less weak, the hydrogen F line also appears, and sometimes C, and the F is brighter than the carbon line. A non-carbonaceous meteorite, like the carbonaceous one, also gives traces of continuous spectrum in the orange, yellow, and green, with a weaker electric discharge.

After describing the changes which took place in Comet Wells, which I have already referred to, Hasselberg writes:—

"The above observations form an interesting addition to our knowledge of the physical peculiarities of the comet, and give a new and

indubitable proof of the inherent luminosity of this body, and also of a greater complication of chemical constitution than former observations had implied. It seems to be a particularly noteworthy fact that the usual cometary spectrum observed first by Tacchini and Vogel from May 22nd to 31st disappeared, while in its stead the bright line spectrum was developed. As this occurrence coincides with the approach of the comet to perihelion, the cause of it may be sought in the rapidly increasing heat of the comet, as thereby on the one hand the sodium present in it was turned into vapour, and on the other hand the electric processes within its mass attained greater vigour. From a discussion of the earlier spectroscopic observations of the comet, and from comparative laboratory experiments of the spectral relations of hydrocarbon, it seems to me very probable that the development of light within this comet chiefly depended on disruptive electric discharges."*

Hasselberg further refers to the experiments of E. Wiedemann on the spectra observed during the passage of an electric current through mixed gases and vapours.

Wiedemann found that when electric sparks were passed through a heated tube containing sodium and a gas like hydrogen or nitrogen, the spectrum consisted solely of lines of sodium. Hasselberg also repeated this experiment, substituting hydrocarbon for hydrogen or nitrogen, and found that the same thing happened. He concludes, therefore, that this demonstrates the electrical origin of the light of comets, since the additional heat due to the approach of the comet to perihelion might certainly bring out the sodium, but could not have caused the hydrocarbon spectrum to disappear.

I would suggest, however, that the changes which took place in Comet Wells can be equally well explained on the supposition that heat alone was in question. The main point to be explained is the disappearance of the carbon fluting spectrum and the appearance of sodium as the comet approached perihelion. With the first increase in temperature, as the comet left aphelion, the occluded compounds of carbon would be driven out of the meteorites constituting the head of the comet, and the spectrum would consequently be that of carbon. At the increased temperature due to further approach to the sun, the carbon flutings would be masked by the increased brightness of the continuous spectrum and by the radiation of other vapours. At the same time a still larger number of meteorites would become incandescent, and vapours of sodium, and possibly also of iron, would distil out. Also since the stones would remain in this condition for a considerable time, sodium vapour would continue to be visible until they had almost ceased to be incandescent.

I may here state that sodium exists only in very small quantities

* 'Astr. Nachr.,' No. 2441.

in iron meteorites, but to a far greater extent in stony ones. A photograph of the arc spectrum of the Obernkirchen meteorite shows barely a trace of D, but the spectrum of a mixture of iron and stones shows it fairly bright.

XI. Conclusions.

I must again refer to the vast difference in the way in which the phenomena of distant and near meteoric groups are necessarily presented to us; and, further, we must bear in mind that in the case of comets, however it may arise, there is an action which drives the vapours produced by impacts outward from the swarm in a direction opposite to that of the sun.

It must be a very small comet which, when examined spectroscopically in the usual manner, does not in consequence of the size of the image on the slit enable us to differentiate between the spectra of the nucleus and envelopes. The spectrum of the latter is usually so obvious, and the importance of observing it so great, that the details of the continuous spectrum of the nucleus, however bright it may be, are almost overlooked.

A moment's consideration, however, will show that if the same comet were so far away that its whole image would be reduced to a point on the slit-plate of the instrument, the differentiation of the spectra would be lost; we should have an integrated spectrum in which the brightest edges of the carbon bands, or some of them, would or would not be seen superposed on a continuous spectrum.

The conditions of observation of comets and stars being so different, any comparison is really very difficult; but the best way of proceeding is to begin with the spectrum of comets, in which, in most cases, for the reason given, the phenomena are much more easily and accurately recorded. But even in the nucleus of a comet as in a star it is much more easy to be certain of the existence of bright lines than to record their exact positions,* and as a matter of fact bright lines, as we have seen, including in all probability hydrogen, have been recorded, notably in Comet Wells and in the Great Comet of 1882.

Allowing for these differences in the conditions of observations, the discussion shows that the changes in the spectrum of a meteor-swarm in the solar system are closely related to those which take place in a swarm outside the solar system.

In both cases, when the number of collisions is just sufficient to render the swarms visible, the spectra are identical, consisting simply of the radiation of the fluting of magnesium at 500.

* "*Observations of Comet III*, 1881, *June* 25.—The spectrum of the nucleus is continuous; that of the coma shows the usual bands. With a narrow slit there are indications of many lines just beyond the verge of distinct visibility."—Copeland, 'Copernicus,' vol. 2, p. 226.

In each case, an increase in temperature is accompanied by the addition of continuous spectrum.

Further condensation of the nebulous swarm results in an apparent star with a spectrum consisting of bright flutings and lines in addition to continuous spectrum, and this condition, we have seen, also has a parallel in cometary spectra.

Still further condensation of the nebulous swarm results in a body of Group II, giving the radiation of carbon and metallic fluting absorption. It has been seen that this is also reproduced in cometary spectra.

The next stage in the history of a nebulous swarm is the formation of a body of Group III, in which the carbon radiation has disappeared, and the metallic fluting- has given way to line-absorption. This, we have seen, was exactly reproduced in the Great Comet of 1882, and in Comet *b*, 1881, to which reference has just been made In the former case, both radiation and absorption lines were recorded, this being due to the repellent action of the sun, as already explained.

The general sequence of phenomena, both in nebulous swarms and comets, may be stated as follows :—

Magnesium (500) radiation.
Carbon and manganese fluting radiation.
Manganese and lead fluting absorption.
Line radiation and absorption.

It is now universally agreed that comets are swarms of meteorites, and hence this connexion between comets and bodies of Groups I, II, and III strengthens the general view, which would have been worthless had the cometary spectra been otherwise. We have, therefore, well-marked species of swarms revolving round the sun exhibiting just the same series of phenomena as marked species of non-revolving ones in space.

Schiaparelli's view, therefore, that comets consist of materials similar in nature to that of which the nebulæ are composed drawn into the solar system by solar attraction, is now abundantly demonstrated by the spectroscopic survey of nebulæ, stars, and comets detailed in my previous papers and in the present one.

[*Note. December 4th.*—Since the above was written, my assistants have made some observations of the nebula in Andromeda, which were suggested by the foregoing discussion. We have seen that some planetary nebulæ give the same spectrum as a comet at aphelion. It appeared that if the nebula of Andromeda were further advanced than a planetary nebula in condensation, it should give a spectrum approximating to one of the more advanced cometary stages which have been already discussed.

The spectrum of this nebula has hitherto been regarded as a perfectly continuous one, but the observations referred to show that there are some parts brighter than others. The spectrum is almost entirely wanting in red and yellow light. In the green there are two maxima, the brightest of which is at wave-length 517, as near as could be determined with the wide slit which it was necessary to employ; the other maximum is near 546. One of the observers, Mr. Fowler, made six independent measures of the maxima on November 20th, and got very nearly the same result each time, comparison being made with the spectrum of a bunsen, and the spectrum of chloride of lead at the temperature of the bunsen. The measurements were repeated on November 27th, with the same result, and on this occasion they were confirmed by another observer, Mr. Coppen. Another brightness near 474, as determined by comparison with the bunsen burner, was also suspected, but it was not so easy to measure as the others.

My suggestion as to the origin of this spectrum is that it is the integration of very slight continuous spectrum, carbon fluting radiation, and the absorption of manganese (558) and lead (546). The citron band of carbon masks and is masked by the manganese fluting, and the absorption fluting of lead causes by contrast the apparent brightness at 546. The brightest maximum is no doubt the brightest fluting of carbon at 517, and the one in the blue, which was suspected, is probably the blue carbon group 468—474.

If these observations are confirmed this nebula is at present at the same stage of condensation as Comet I, 1868, on April 29th (p.p. April 20th), which must be regarded as a pretty advanced cometary stage, seeing that it was observed so near perihelion and that the perihelion distance was small.

The discussion of the observations of Nova Andromedæ, which is not yet completed, shows that there were bright lines in exactly the same positions as the brightnesses which have now been determined in the nucleus of the nebula. The appearance of the Nova was therefore probably due to increased temperature due to collisions taking place between the sparser outliers of the swarm composing the nebula and the external swarm which came in contact with them. The view of the Nova's probable connexion with the nebula is therefore greatly strengthened by this inquiry.]

[*Note added January* 8, 1889.—If it be conceded that the tails of comets are in part composed of hydrogen and gaseous compounds of carbon, an explanation seems to be afforded of many recorded phenomena, among which may be mentioned—

I. The absence of carbon and oxygen from the sun;

II. The presence of hydrogen in the atmosphere of the hottest stars;
III. The presence of carbon in stars on cooling;
IV. The decreasing densities of planets and satellites outwards.

I hope shortly to be able to communicate the result of some experimental work, which is now going on, which may throw light upon this subject.]

[*Note added January* 14, 1889.—Since the above was written, I have come across some observations of Comet C, 1886, made by Mr. Sherman* on May 26th and 28th and June 4th. The perihelion passage of the comet occurred on June 6th, so that all the observations were made near perihelion, when the comet was pretty hot. Unfortunately, the individual observations are not recorded, and we are therefore unable to trace the sequence of spectra. Seven loci of light were observed, and four more were strongly suspected. The wave-lengths given are 618·4, 600·6, 567·6, 553·7, 545·4 (suspected), 535·0 (suspected), 517·1, 468·3, 433 2, 412 9 (suspected), and 378·6 (suspected).

My suggestion as to the origin of this spectrum is that it was the integration of hot carbon and hydrocarbon (431) radiation, cool carbon absorption, manganese absorption, and lead absorption; *i.e.*, it was similar to Coggia's Comet on June 13th (see p. 176), with the addition of lead (546). The maximum at 618 4 was in all probability the iron fluting, and that at 567·6 was probably the second fluting of lead (568). This leaves the loci at 600·6, 535·0, 412·9, and 378·6 unexplained, the latter three being only suspected.]

II. "ON SOME EFFECTS PRODUCED BY THE FALL OF METEORITES ON THE EARTH."

PART I.—FALLING DUST.

In my paper of November 17, 1887, I stated that Professor Newton and others have calculated that not less than twenty millions of meteorites, each large enough to present us with the phenomenon of a shooting star visible to the naked eye, enter our atmosphere daily. If this be conceded, the upper parts of our atmosphere must be constantly charged with meteoric dust, whether oxidised or not, in a state of suspension, while it is possible that the earth encounters particles finer than those which produce the phenomena of falling stars.

The only means open to us of determining the presence or absence

* 'Amer. Journ. Sci.,' vol. 32.

of this dust in the higher regions of the air is by spectroscopic observations of the atmosphere containing it when it is rendered luminous by electrical discharges. It becomes necessary, therefore, to make a thorough investigation of the spectrum of the aurora borealis from the point of view that meteoric dust, if it exists, is likely to assert itself in any electrical excitation of the atmosphere.

It is now many years since the idea was first thrown out that the aurora was in some way connected with shooting stars. The connexion was first suggested by Olmsted in 1833.*

M. Zenger, in a catalogue of auroræ observed from 1800 to 1877, showed an apparent connexion between the brightest displays and the appearance of large numbers of shooting stars, and M. Denza noted the same connexion on November 27, 1872, and remarked that he had noticed it before.

In spite of these ideas, however, even after the chemical nature of shooting stars was known, observers have in the main contented themselves with making comparisons of the aurora spectrum with the spectrum of air under different conditions of temperature and pressure.

It has never been possible, however, to reconcile the aurora spectrum with any known spectrum of air. Some observers are of opinion that the lines seen in the aurora coincide with air-lines, but have different intensities, and they attempt to overcome this difficulty by assuming that the aurora spectrum is produced under conditions which we are unable to imitate in our laboratories.

When we recognise the importance of considering the possible existence of meteoric dust in the atmosphere, a comparison with the spectra of uncondensed meteor-swarms is at once suggested, for the more my researches advance the more does dust rather than large meteoritic masses appear to be in question.

The result of a preliminary comparison with γ-Cassiopeiæ and with the bands in Dunér's stars was communicated to the Royal Society on January 9, 1888. The tables which I then gave show that there is probably a very intimate relation between the spectrum of the aurora and those of meteor-swarms.

The further inquiry into the recorded observations to which I have subsequently to refer, seems entirely to justify the suggestion then put forward, and I now propose to show what progress has been made in attacking what has always been regarded as a difficult subject. I will first, however, briefly refer to the observations and comparisons which have been previously made, and discuss them in chronological order.

It is necessary to state that the existing observations of aurora spectra show such great differences of wave-length for what are

* 'Amer. Journ. Sci.,' vols. 35 and 36.

probably the same lines, that it is somewhat difficult to assign origins for the lines. These discrepancies occur not only in the measures made by different observers, but in those made at different periods by the same observer. Further, the individual observations are seldom recorded, but in place of them are given the means of several observations, and in some cases the means have been obtained by throwing together lines which are very far apart. At best, therefore, it is only possible to suggest the most probable origins of the lines and bands seen.

The object of the present paper is therefore mainly to direct further inquiries.

I. Early Observations.

Ångström's First Observations.

The spectroscope was employed in investigating the nature of the aurora spectrum by Ångström in 1867.* He found that the light was almost perfectly monochromatic, the spectrum consisting mainly of a yellow-green line at a wave-length given by him as 5567. With a wide slit other faint bands were visible.

The note is so short that I give it in full; translated it reads thus:—

"From the time of Franklin's memorable observations on electricity up to the present there has been a perfect agreement between the actions of this natural force and those of frictional electricity, that it was easy to foresee that the spectrum of lightning must be the same as that produced by the ordinary electric discharge in air. The observations made by M. Kundt have perfectly proved this. The two phenomena of the aurora borealis and of terrestrial magnetism being so closely connected with each other, that the appearance of the aurora is always accompained by disturbances of the magnetic needle, it might be supposed that the aurora borealis was only an electric flash, which is however not the case. During the winter of 1867–68 I was able several times to observe the spectrum of the luminous arc which borders the dark segment, and is always present in faint auroræ. Its light was almost monochromatic, and consisted of one bright line, on the left of a group of calcium lines. I determined the wave-length of the line which was equal to $\lambda =$ 5567. Beyond this line the intensity of which is relatively great, I observed also, by increasing the width of the slit, traces of three very faint bands which extended almost to F. On one occasion only, where the luminous arc was agitated by undulations which changed its form, I saw the regions in question lighted momentarily by some faint spectral lines; but considering the lack of intensity of the

* 'Spectre Normal du Soleil,' 1868, p. 41.

rays, it may still be said that the light of the luminous arc is sensibly monochromatic.

"Here is a circumstance which gives this observation on the spectrum of the aurora borealis a greater and even cosmic importance. During a week of the month of March, 1867, I succeeded in observing the same spectral line in the zodiacal light which had then an extraordinary intensity for the latitude of Upsala. At last, during a starlight night, the whole heavens being in a manner phosphorescent, I found traces of it even in the faint light emitted from all parts of the firmament. A very remarkable fact is that the line in question coincides with none of the known lines in the spectra of simple or compound gases, at least so far as I have studied them at present. It follows from what I have said that an intense aurora borealis, such as may be observed above the polar circle, will probably give a more complicated spectrum than that which I saw. Supposing that to be the fact, it may be hoped that in the future it will be possible to explain more easily the origin of the lines found and the nature of the phenomenon itself. Not being able to give this explanation at present, I propose to return to it another time."

Zöllner's View.

In the 'Report to the Royal Saxon Academy of Sciences,' October, 1871, Zollner expressed the opinion that the temperature of the incandescent gas of the aurora must be very low. He affirms that the spectrum does not correspond with that of any known substance, and suggests, therefore, that it may be one given by air under some peculiar condition which cannot be experimentally reproduced. (A translation of Zöllner's paper is given in the 'Philosophical Magazine,' vol. 41, 1871, p. 122.)

Vogel's Views.

Vogel also makes the same affirmation, and comes to the same conclusion as Zöllner, namely, that the spectrum of the aurora is one which cannot be artificially produced. He suggests that it may be the integrated spectrum of several layers which exist under different conditions ('Reports of the Royal Saxon Academy of Sciences,' 1871).* He points out that the characteristic line in the aurora spectrum observed by Ångström is coincident with a very faint line of nitrogen. That this line should appear in the aurora spectrum with enhanced intensity he regards as quite consistent with the known variability of gas spectra under various conditions of temperature and pressure. He also points out the possible coincidence of one of the lines with a line in the negative-pole spectrum of

* A translation of Vogel's paper is given by Capron ('Auroræ,' p. 194).

nitrogen at wave-length 5224, of another with an oxygen line at 5189, and of another with the strong nitrogen line 5004. The red line in the spectrum he regards as having the same origin as the group of lines in the spectrum of nitrogen which extends from 6213 to 6620, and brightens towards the violet end, the change in appearance being due to the faintness of the aurora. This, however, is not likely to be the case, as the red line has been seen both bright and sharp (R. H. Proctor, "Aurora," 'Encycl. Brit.,' 9th edit.).

In the same paper, Vogel shows the close coincidences between the aurora lines and lines in the spectrum of iron, but considers it more in accordance with probability to regard the aurora spectrum as a modification of the spectrum of atmospheric air.

Ångström's further Observations and Conclusions.

In a later paper ('Nature,' vol. 10, p. 210), Ångström arrives at conclusions which may be thus briefly stated :—

(1.) That the aurora has two different spectra, one consisting of the characteristic line, and the other consisting of the fainter lines.

(2.) That the coincidences of the bright green line with a faint line in the spectrum of air, as determined by Dr. Vogel, is purely accidental, and also that there is no coincidence of any importance with any member of the hydrocarbon group in which it falls.

(3.) That the bright line is probably due to fluorescence or phosphorescence.

(4.) That Vogel's theory of unknown conditions of temperature and pressure being competent to produce the change from the ordinary experimental spectrum of air to that given by the aurora, is inadmissible. (Ångström regarded the spectrum of a gas as invariable.)

(5.) That moisture may be neglected in considering the nature of the aurora spectrum.

He describes an experiment on a glow equivalent to the glow of the negative pole of an air vacuum-tube, in which the spectrum obtained showed close coincidences with three faint lines in the aurora spectrum. A layer of phosphoric anhydride is spread over the bottom of a flask fitted with platinum wires; after exhaustion with an air-pump, the current from an induction coil is passed between the two platinums. The flask then becomes filled with a violet light like that which, under ordinary conditions, only appears at the negative pole. The spectrum of this light shows the following close coincidences with that of the aurora :—

Auroræ ...	Barker........	431	470·5	—
	Vogel.........	—	469·4	523·3
	Ångström	—	472·0	521·0
	Lemström.....	426·2	469·4	523·5
	Means	428·6	470·3	522·6
Violet light................		427·2	470·7	522·7

Although this coincidence is rather striking, it must be remembered that there are other strong bands in the spectrum of the negative pole which do not appear in aurora spectra. As mapped by Hasselberg, the spectrum of the negative pole consists of a series of bright flutings shading off towards the violet, the brightest edges of them being at wave-lengths 419·8, 423·6, 427·8, 451·5, 455·4, 459·9, 465·1, 470·8, these are all of equal intensities.* (See fig. 16.)

Capron remarks that "if the violet-pole glow spectrum is to represent the aurora spectrum, it must be under conditions different from those by which it obtains in dry-air vacuum-tubes or flasks at ordinary temperatures" ('Auroræ,' p. 126).

There can, therefore, be little doubt that the aurora spectrum has nothing in common with the negative-pole spectrum of nitrogen, and that the three close coincidences noted by Ångström are merely accidental.

With regard to Ångström's objection to Vogel's theory that to view the aurora spectrum as a spectrum of air under unknown conditions is inadmissible, we now know that gas spectra are not so invariable as Ångström supposed; but still we have no right to assume that any particular change is possible until we can prove it experimentally, or at the very least, prove an approach to such a change. If we assume that any change may take place in any spectrum, we upset the whole basis of spectrum analysis.

Comparison of the Aurora Spectrum with the Negative-pole Spectrum of Oxygen.

The negative-pole spectrum of oxygen, as mapped by Schuster ('Phil. Trans.,' 1879, Part I) consists of four broad bands, the two brightest having the following positions :—

5205·0 – 5292·5	Brightest part	5255
5552·8 – 5629·6	Brightest part	5586

Under great dispersion, these bands break up into series of lines.

* 'Mémoires de l'Académie Impériale des Sciences de St. Pétersbourg,' Series 7, vol. 32, No. 15.

The proximity of the brightest part of one band (5586) to the aurora line is notable, but considering that the aurora line is always sharp, Schuster concludes that there is no connexion between the spectrum of the aurora and that of the negative-pole glow of oxygen (quoted by Capron, 'Auroræ,' p. 130).

Comparison with the Spectrum of Hydrogen.

Similarly, all attempts to identify the spectrum of the aurora with that of hydrogen, another constituent of our atmosphere (in the form of water vapour), have failed. On this point Capron remarks:—"No principal line, and one subsidiary line only,* actually coincide with the aurora spectrum, this last being that to which Dr. Vogel assigns an identical wave-length, viz., 5189" ('Auroræ,' p. 109).

That this coincidence is of no importance is obvious when it is remembered that there are a great number of such lines in the spectrum of hydrogen, and that no experiments have been recorded indicating that this line is more persistent than the others.

Comparison with the Spectrum of Phosphoretted Hydrogen.

Next in importance to comparisons of the aurora spectrum with air spectra is the comparison with the flame of phosphoretted hydrogen, in connexion with Ångström's suggestion that the characteristic green line may be due to phosphorescence or fluorescence. The spectrum of phosphoretted hydrogen consists of several bands, the centres of the four brightest being at 526·3, 510·6, 560·5 and 599·4 (Lecoq de Boisbaudran, 'Spectres Lumineux,' p. 189). These bands brighten when the flame is artificially cooled, especially the less refrangible ones.

On this subject, Capron says: "Having regard to the near proximity of the phosphoretted hydrogen band to the bright aurora line, to the circumstance of this band brightening by reduction of temperature (a phenomenon probably connected with ozone), to the peculiar brightening of one line in the green in the "aurora" and "phosphorescent" tubes (the phosphorescent tubes probably containing O), and to the observed circumstance that the electric discharge has a phosphorescent or fluorescent afterglow (isolated, I believe, by Faraday), I feel there is strong evidence in favour of such an origin to the principal aurora line, if not to the red line as well" ('Auroræ,' p. 126).

But the mere fact of one of the phosphoretted hydrogen bands, and that only the third in order of brightness, falling near the characteristic aurora line cannot be supposed to be anything more

* The subsidiary lines of hydrogen constitute what I described as the structure-spectrum of hydrogen in my paper of November 17, 1887.

FIG. 16.—Diagram showing that the Aurora Spectrum is not a spectrum of nitrogen or oxygen.

than accidental, unless the absence of the two brightest bands can be explained. As this cannot be done, the suggestion may be disregarded.

The information given about the green line seen in the phosphorescent tube by Capron is insufficient for any conclusions to be founded on it.

Fig. 16 is a map showing that the aurora spectrum is not that of the negative or positive pole of nitrogen, or any spectrum of oxygen, although there are some apparent coincidences. The intensities of the lines and bands in the spectra are indicated by lengths, the longest being the brightest. The map shows that lines or flutings as bright as or brighter than those which have been supposed to coincide with lines in the aurora are absent from the aurora spectrum. The probable meteoritic origins, which I shall have to refer to in detail later on, are shown at the bottom of the map.

Groneman's reference to the Meteoric Dust Theory.

So far we have had chiefly to deal with theories in which the aurora spectrum is regarded as being inseparable from that of atmospheric air, but we have next to consider one which, if true, would give a totally different origin.

In 1874, Groneman ('Astr. Nachr.,' No. 2010) resuscitated the theory of Olmsted that the aurora has its origin in the fall of incandescent meteoric dust.* The iron particles are regarded as being competent to produce the magnetic phenomena which accompany auroræ, and as being consistent with their geographical distribution. This theory, however, was not received very favourably, because it left the spectroscopic phenomena as far from a solution as ever. Thus, Capron remarks ('Auroræ,' p. 170) that "if auroræ were composed of incandescent glowing meteors, it would be reasonable to expect to find in the spectrum the lines of iron, a metal constituting so prominently the composition of meteorites. No connexion between the iron and the aurora spectrum is, however, proved; though it may be suspected. The iron spectrum contains so many lines that some may, as a mere accidental circumstance, closely agree with the aurora lines." Vogel also considers that we are not entitled to regard the close coincidences of the aurora lines with some of the iron lines as complete evidence of iron vapour, until we have succeeded in showing by experiments that the relative intensities of the iron lines are subject to great changes; and in this way to account for the appearance of faint lines in the aurora spectrum, or, on the other hand, to account for the absence of the strongest lines. I shall show subse-

* This theory was subsequently discussed in an appendix to the 'Memorie della Società degli Spettroscopisti Italiani,' 1878.

quently what experiments have now conclusively proved the presence of iron.

Mr. Capron's Conclusions.

In reviewing the above theory to explain the origin of aurora up to 1879, Mr. Rand Capron makes the following statement: "As the general result of spectrum work on the aurora up to the present time, we seem to have quite failed in finding any spectrum which, as to position, intensity, and general character of lines, well coincides with that of the aurora. Indeed, we may say we do not find any spectrum so nearly allied to portions even of the aurora spectrum as to lead us to conclude that we have discovered the true nature of one spectrum of the aurora (supposing it to comprise, as some consider, two or more). The whole subject may be characterised as still a scientific mystery." ('Auroræ,' p. 171.)

II. *Lemström's Observations.*

The next contribution to our knowledge of aurora spectra of any importance is that of Lemström's ('L'Aurore Boréale,' 1886). All previous observers who attempted to identify the spectrum of the aurora with that of atmospheric air failed to do so, but Lemström asserts (p. 158) that the twelve lines which have been recorded in aurora spectra are nearly all seen in the spectrum of a Geissler tube containing the same gases as those constituting our atmosphere. The differences in the relative intensities he believes to be due to conditions of temperature and pressure.* Although the auroral line (wave-length 557) does not agree perfectly with the line at 558 seen in the spectrum given by his *appareil de l'aurore boréale* (air vacuum-tubes illuminated by sparks from a Holtz machine), he regards the atmospheric origin of the aurora spectrum as completely demonstrated. He states (p. 138) that the characteristic line of the aurora spectrum is always seen in the light produced by the discharge of an electric current (by means of his *appareil d'écoulement*) from the top of a mountain. He gives a table of auroral lines compared with the lines in the spectra of rarefied air, as observed by himself, and by Vogel and Sundell under other conditions. The air lines recorded by Vogel nearly all coincide with lines recorded as oxygen lines by Schuster ('Phil. Trans.,' 1879); but it is important to note that some of the strongest lines mapped by Schuster are absent from Vogel's list (see fig. 16). So that, even if we allow that some of the aurora lines fall near lines of oxygen, the absence of the brightest oxygen lines from the spectrum is sufficient evidence for us to conclude

* "Si l'on se demande pourquoi on ne voit point dans l'aurore polaire toutes les raies existant dans ces gaz, l'expérience répond que les raies des gaz changent selon la température et la pression de ces gaz." ('L'Aurore Boréale,' p. 158.)

safely that we are not dealing with the line spectrum of oxygen. We have previously seen that it is not the negative-pole spectrum of oxygen.

In the same table ('L'Aurore Boréale,' p. 92), the aurora lines are compared by Lemström with some of the lines or bands observed by himself in the spectrum of rarefied air. The air lines which he gives all agree in position with some of the nitrogen flutings mapped by Hasselberg ('Mémoires de l'Académie Impériale de St. Pétersbourg,' Series 7, vol. 32, No. 15). One of them is at wave-length 558, and this he believes to be coincident with the aurora line 557. The intensity of the line is not given, but Hasselberg gives it as a comparatively feeble fluting at 557 (see fig. 16). Considering the absence of the brightest nitrogen flutings from the spectrum of the aurora, the supposed coincidences between some of Lemström's rarefied air lines and lines in the aurora spectrum, which are far from perfection, may be disregarded.

The same objections apply to the lines in the rarefied air spectrum which have been recorded by Sundell; those which fall anywhere near lines in the aurora are comparatively faint flutings or lines in the spectrum of nitrogen; at all events, flutings of the same or greater intensities are absent, and there is no evidence to show that the coincident ones retain their brightness as the others fade.

Lemström then leaves the origin of the aurora spectrum as uncertain as ever. There is no evidence to show that it is a spectrum of air, or, indeed, of any other gas. If it be a spectrum of air, it is one which has never been obtained experimentally, and one which can only be put forward by making unphilosophical assumptions and carefully avoiding experiments.

III. *Gyllenskiöld's Observations and Conclusions.*

Still later observations of the aurora which have been published are those made at Cape Thordsen by M. Carlheim-Gyllenskiöld.* Two lists of lines are given, one from observations made with a Hofmann spectroscope, and the other from observations made with a Wrede spectroscope. The lines in the first list extend from blue to red, and those in the second list from green to violet. The individual observations of different auroræ with the lines observed in each are given. 36 auroræ are recorded in which only 1 line was visible, 15 in which there were only 2 lines, 6 with 3 lines, 15 with 4 lines, 5 with 6 lines, 4 with 7 lines, 1 with 8, 1 with 9, and 1 with 10 lines, so that altogether, no less than 84 observations are recorded.

The total number of lines seen were 32. Gyllenskiöld's main conclusions are:—

* 'Observations faites au Cap Thordsen, Spitsberg, par l'Expédition Suédoise.' Vol. 2, 1.—"Aurora Borealis," par Carlheim-Gyllenskiöld.

(1.) That 16 of the aurora lines nearly coincide with air lines, 8 with the positive-pole spectrum of nitrogen, 4 with the negative-pole spectrum of nitrogen, and 3 with lines of hydrogen.

(2.) That the aurora spectrum greatly resembles that of lightning, and regards it as consisting of several superposed spectra. The variable character of the spectrum is accounted for by the absence sometimes of one, sometimes of another, of these elementary spectra.

(3.) The brightness of the aurora, according to M. Gyllenskiöld, does not depend upon the energy of the electrical discharge which produces it, but upon some cause with which we are not acquainted.

Note.—It is not out of place to suggest that the brightness of the aurora may depend upon the varying quantities of meteoric dust in the atmosphere at different times.

(4.) Two kinds of aurora are distinguished, viz., red ones and yellow ones. In the former, the positive-pole spectrum of nitrogen is predominant, while in the latter the negative-pole spectrum is predominant. Laboratory experiments have shown that the positive-pole spectrum of nitrogen is given by dense moist air, while the negative-pole spectrum is given by rarefied dry air; and Gyllenskiöld suggests that yellow auroræ are formed in the higher parts of the atmosphere, and the red ones in the lower layers.

(5.) That the observations bear out Ångström's suggestion that some of the bands belong to the negative-pole spectrum of nitrogen. He says:—"Nos observations confirment donc l'opinion d'Ångström, que les bandes faiblement lumineuses de l'aurore boréale appartiennent au spectre du pôle negatif; auxquelles les bandes et les lignes de l'azote se joignent dans certains cas." He observes that the characteristic line of the aurora appears in company with the negative-pole spectrum, and says it is probable that some of the more refrangible bands of the positive-pole spectrum also appear at the same time. Both the positive and negative-pole spectra are very rich in violet and ultra-violet rays, and Gyllenskiöld's observations support Ångström's view, that the characteristic line is due to the fluorescence of oxygen produced by the violet light of the negative pole.

This fluorescence, however, cannot be reproduced in experiments with Geissler tubes, and M. Gyllenskiöld concludes that the origin of the characteristic line still remains unexplained, but he suggests that its origin may eventually be discovered by investigation of the fluorescent spectra of various chemical substances.

The characteristic aurora line therefore remains unexplained by M. Gyllenskiöld. As regards the remaining lines, he states that sixteen nearly coincide with air lines, but it is important to note that these are not the sixteen strongest air lines. Some of the lines fall near to bands in the positive-pole spectrum of nitrogen, as Gyllenskiöld points out, but equally strong or stronger bands are not seen in the

aurora, so that the coincidences are only accidental. The same applies to the bands in the negative-pole spectrum.

'Like Lemström, then, Gyllenskiöld makes no advance as regards the origin of the spectrum of the aurora, but at the same time it is only fair to acknowledge the value of the observations.

I have next to refer to my own observations and comparisons.

IV. *The Sequence of the Flutings and Lines seen in a large Tube at different Stages of Pressure.*

In order to demonstrate that the aurora spectrum does not coincide with the vacuum-tube spectrum of air, I have made a series of observations of an end-on air vacuum-tube, about 5 feet long and 2 inches in diameter. The tube was arranged as in fig. 17, one end being connected with the Sprengel pump, and the other with a piece of glass tube by means of mercury joints. The latter tube was connected with a hand air-pump to save time in exhausting. After partial exhaustion the tube was sealed off with a blowpipe, and the exhaustion completed with the Sprengel. The slit of the spectroscope was

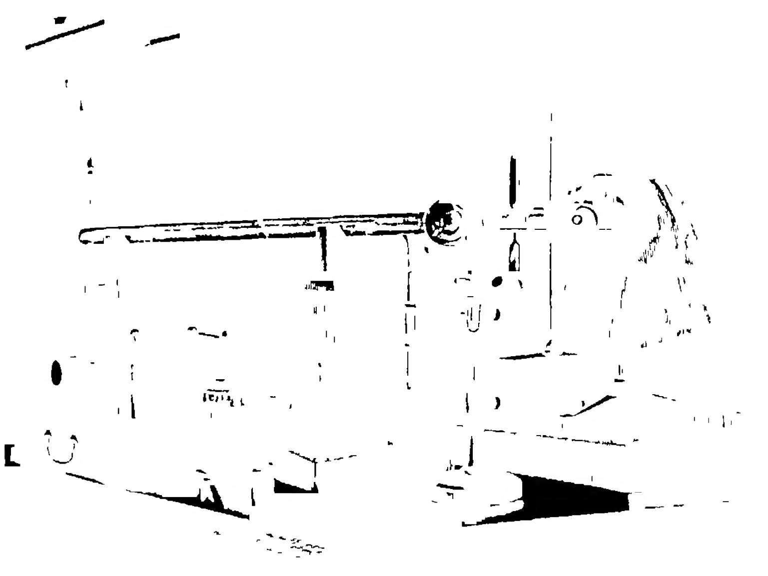


Fig. 17.—Large end-on vacuum-tube, arranged for an observation of the Spectrum of air at varying pressures.

placed close to the bulb at the end of the tube (fig. 17). The diagram also shows a Geissler tube arranged for comparison.

FIG. 18.—Map showing the sequence of Spectra in a large air-vacuum tube as the pressure is reduced.

When the spark first passed only a few of the strongest nitrogen flutings in the violet were visible, but as the pressure was reduced, the spectrum gradually extended towards the red. A line of oxygen near 5316 was visible in the early stages, but it afterwards disappeared. At the most luminous stage, nothing but nitrogen flutings were visible. After a time the nitrogen flutings dimmed, and low-temperature flutings of carbon appeared. Then the F line of hydrogen appeared, and a little later the C line. Later still, the hydrogen line at G also appeared. With the further dimming of the nitrogen flutings, an oxygen line at 471 brightened, being sometimes as bright as the F line, and brighter than the carbon flutings. The whole spectrum then became very faint, but as the line at 471 dimmed, another oxygen line at 465 appeared. Ultimately, the glow was so faint that only a few of the nitrogen flutings were visible.

The sequence of the various flutings and lines is shown in fig. 18. Below the various air spectra the principal lines of the aurora spectrum are given for comparison. The spectra of nitrogen, carbon, and hydrogen are given as a key to the spectra observed. It should also be stated that the line near 5316 is an oxygen line. I am now working at this line. It will be seen at a glance that there is only one coincidence with one of the most persistent flutings, which are all that need be considered. Since equally persistent flutings are not present in the spectrum of the aurora, this coincidence is obviously of no importance.

V. *Comparison with Uncondensed Meteor-swarms.*

In my preliminary communication I indicated the remarkable coincidences between the lines in the spectrum of the aurora and the bright lines in the spectrum of γ-Cassiopeiæ, and also with the absorption-bands in bodies of Group II. These bodies are uncondensed swarms of meteorites at a comparatively low temperature, and hence the comparison suggests the probable meteoritic origin of the spectrum of the aurora.

I have since extended the tables which I then gave, and excluding for the present Gyllenskiöld's observations, they now stand as below:—

Table of Wave-lengths of Auroral Lines.

Barker			431	470	482	502	517			533			562		623
Smyth			432	464	480			520					558		635
Zöllner			435												628
A. Clerke					485				532						
Herschel									531						
Backhouse			431	463		501	516 5		532					606	
Lord Crawford					more ref than F			523							
Proctor (R. H.)					,,		519	523			539		557		630
Vogel				463–469											
Ellery													560		635
O. Struve													555		
Ångström				472	487	501		521					557		
Lemström	411	426		469–471		499		525					557		
German N. P. Ex													557		
Respighi													557		
Peirce			431	464	486			520	531			545	557		
Winlock												544	557		
Wijkander		428	436	469	484	500		524		536			557		
Oettingen		424		466									555		630
Probable origin	(?)		CH	C hot	C cool	Mg	C hot	Mg	*	Tl	Mn	Pb (1)†	Mn (1)†	Fe (1)†	‡
Wave-lengths of probable origin			434	467–474	483	500	516–5	520·1		535	540	546	558	615	
Dunér's bands				460 474 10	477 485 9	495 503 8	516	521 7				550 545 5	559 564 4	616 627 2	

* Coronal line. † This means brightest fluting.

‡ Origin not determined, but a line near this position is seen in the spectrum of the Limerick meteorite.

The following table shows the above figures in another form and includes the bright lines recorded in γ-Cassiopeiæ:—

Aurora (means).	Dunér's bands.	Bright lines in γ-Cassiopeiæ.	Probable origin.	Wave-length of probable origin.
411	..	..	..	..
426	..	..	..	..
432	..	..	CH	431
..	..	462·3	Sr	460·7
474—478	460—474 (10)	..	C (hot)	474
484	477—485 (9)	..	C (cool)	483
500	495—503 (8)	499	Mg	500
516·5	516—521 } (7)	516·7	C (hot)	516·5
522	..	..	Mg	520·1
531	..	531	Coronal line	
535	..	..	Tl	535
539	..	542·2	Mn	540
545	545—550 (5)	..	Pb (1)	546
558	559—564 (4)	555·7	Mn (1)	558
..	585—595 (3)	586	Mn (2)	586
606	..	..	..	..
620	616—630 (2)	..	Fe	615
630	..	635·6	*	..

The chemical substances indicated by Dunér's bands, and by the lines in γ-Cassiopeiæ, are those constituents of meteorites which are volatilised at the lowest temperatures, namely, magnesium, manganese, and lead. Besides these there are compounds of carbon, which, when rendered incandescent, give the carbon flutings.

In discussing the meteoric dust theory, as first enunciated by Olmsted during the display of 1833, spectroscopists lost sight of the importance of considering the volatility of meteoric constituents, instead of quantities. Iron exists in great quantity in meteorites, and was naturally the first thing to be expected in the aurora spectrum, supposing it to be a meteoritic phenomenon. But, as I pointed out in my paper to the Royal Society on November 17, 1887, experiments on the luminous phenomena seen at low temperatures show that if magnesium, manganese, and lead are present in meteorites, they will be indicated in the spectrum before the iron.

The experiments have shown that a very small percentage of manganese is sufficient to render the first fluting (558) visible. It is the first fluting seen when ordinary iron wire is volatilised in

* This line is seen as a pretty bright line in the spectrum of the Limerick meteorite, but its origin has not yet been determined, although comparisons have been made with most of the common elements. So far, it has not been observed in any other meteorite.

the oxy-coal-gas flame, and even with the purest electrolytic iron prepared by Jacobi and by Professor Roberts-Austen it is visible before the iron lines. The importance of this fluting in this discussion cannot therefore be overrated.

The aurora being a low-temperature phenomenon, we should expect to find in its spectrum, lines and remnants of flutings seen in the spectra of meteorites at low temperatures, the manganese fluting being the most prominent for the reason before stated.

The characteristic line of the aurora is the remnant of the brightest manganese fluting at 558. Ångström gave the wave-length of the line as 5567, and since then many observers have given the same wave-length for it, but probably without making independent determinations. Piazzi Smyth, however, gives it as 558, which agrees exactly with the bright edge of the manganese fluting. R. H. Proctor also gives the line as a little less refrangible than Ångström's determination. He says:—"My own measures give me a wave-length very slightly greater than those of Winlock and Ångström" ('Nature,' vol. 3, p. 468).

Gyllenskiöld's measures with the Wrede spectroscope also give 5580 as the wave-length of the characteristic line. I feel justified, therefore, in disregarding the difference between the wave-length of the edge of the manganese fluting and the generally accepted wave-length of the aurora line.

The line of manganese at 540, which is seen in the spectra of many of the "stars" with bright lines, has been recorded in the aurora by Vogel.

The remnants of the two magnesium flutings seen in bodies of Group II, at wave-lengths 500 and 521, are also seen as lines in the aurora. In addition to these, there is sometimes the lead fluting at 546, corresponding to Dunér's band 5, and probably also the green line of thallium at 535, as indicated in the tables.

Four lines in the aurora spectrum are probably due to carbon. The first is at 516·5, the brightest fluting seen in the spectrum of a bunsen burner; I have previously described this as a high-temperature fluting, but the term is only relative. The second is the low-temperature fluting at 483, which has been recorded by several observers. There is probably also the high-temperature carbon group beginning at 474, the maximum light of which is about 469. Vogel records it as a band extending from 463 to 469, and Lemström as 469 to 471. These observations, therefore, justify us in regarding this as a band, and if we take the readings of the other observers as the wave-lengths of the part of maximum brightness, we get the mean reading of the maximum as 467·5. This agrees as well as can be expected with the true wave-length of the maximum, 468. The hydrocarbon fluting at 431 has probably also been seen.

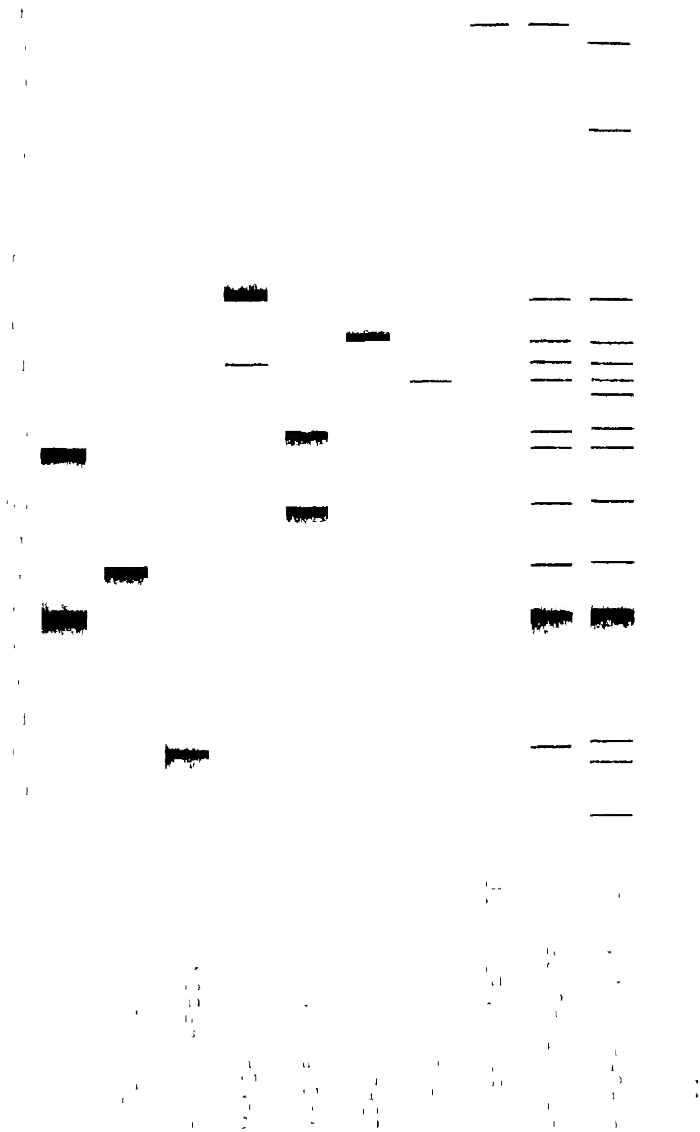


Fig. 19.—Map showing the probable origin of the Spectrum of the Aurora.

Fig. 19 shows how the aurora spectrum can be built up from the lowest-temperature spectra of manganese, magnesium, lead, and thallium, and the brightest flutings of carbon.

When the temperature is increased iron (615) sometimes flashes in. This was particularly noticed in the Norwegian observations, to which I have subsequently to refer.

VI. *Further Discussion of Gyllenskiöld's Observations.*

If, in discussing Gyllenskiöld's observations, we limit ourselves to those cases in which not more than four lines were recorded, we find that with a few exceptions, the lines seen were lines which are brightest in the spectra of meteorites at low temperatures. It might at first sight be expected that when only a few lines are seen, they ought to be the same in every case. There are variations, however, which in all probability are due to differences in composition of different groups of meteorites.

The following tables contain all the observations in which not more than four lines were recorded. The probable origin of each line is also stated. Some of the lines have been arranged in different columns, as the discussion has suggested.

It will be observed that the characteristic line was seen alone eight times by Gyllenskiöld out of the thirty-eight observations recorded in the first table.

Out of the total number of seventy-six observations in the tables, the line of manganese at wave-length 540, which is seen in the spectra of many of the "bright line stars," was seen alone on two occasions, and six times in company with other lines.

The first fluting of lead, at wave-length 546, occurs alone three times, is twice associated with the thallium line, and occurs six times along with other lines.

The remnant of the magnesium fluting at 500 occurs alone only once, but that at 521 occurs alone six times.

The first fluting of carbon, at 517, occurs alone three times, and twice in company with other lines. The carbon band extending from 468 to 474 occurs alone four times, and six times with other lines. The low-temperature fluting of carbon at 483 only occurs once, and is then alone. The first iron line at 579 occurs alone twice, and six times along with other lines. When we get iron apparently without manganese 558 it is probably due to masking of 558 by continuous spectrum. The green line of iron at 527 occurs alone seven times, and thirteen times in company with other lines.

The thallium line appears alone only once, but in company with other lines it appears fifteen times.

H	?	?	C (hot)	C(cool)	?	Mg(1)	?	Ba(2)	Mg(2)	Fe(3)	Tl(1)	Mn	Pb(1)	Fe(1)	?	Meteoric origins.			
				483		500		515	521	527	535	540	546	579		Wave-lengths of probable origins.			
																			hours.
									5249							1	Nov.	11	22
									5249							1	"		"
										5283						1	Dec.	11	12
										5285						1	"		"
										5253						1	"		"
										5290						1	"		"
			4799													1	"		18·25
										5277						1	"		20
											5338					1	"		21·45
													5483			1	"	12	17·30
													5451			1	"	20	16·5
												5417				1	"	26	18·30
				4834												1	"	28	11
			4716													1	"	29	11·55
			4723													1	"		21·12
										5296						1	"	30	0·5
									5217							1	"		21·17
						4992										1	Jan.	2	14·25
												5411				1	"		20·30
			4696													1	"	6	18·9
			4756													1	"	6	18·9
4088																1	"	10	22·19
					4925				5218							2	Nov.	11	20
					4937					5273						2	"		22
										5295	5373					2	Dec.	11	12
							5065				5326					2	"		20
											5319		5451			2	"	12	16·25
											5391		5483			2	"		16·50

H	(hot) C(cool	Ba(2)	Mg(2)	Fe(3)		Mn	Fe(1)					ori
					535	540			Wa	ngths of le origins		
									2	Dec.	29	11·45
									2	”		20
									2	”	30	21·17
									3	”	26	18·30
									3	Jan.	2	14·40
								5952	4			20·30
									4		6	18 9
				5274					4			”
									4		10	23·2

Ba(2)	C(1)	Mg(2)	Fe(3)	?	Tl(1)	?	Mn(2)	Pb(1)	Ba(1)	Mn(1)	Pb(2)	Fe(1)	?	Fe(2)	Lime-rick Met.	Meteoric origins.			
515	516·4	521	527		535		540	546	553	558	568	579		615	634	Wave-lengths of probable origins.			
																			hours.
										5582						1	Dec.	23	12
										5579						1	"		"
										5575						1	"		"
										5553						1	"		20
		5237														1	"		21
										5546						1	"	25	12·5
										5563						1	"		12·6
										5564						1	"	28	11
		5242														1	"	29	21·25
										5560						1	Jan.	6	18·9
								5490								1	"		23·2
												5797				1	"	8	8·30
												5753				1	"	10	22·2
			5265													1	"		23·5
	5178															1	"	12	23·20
	5157															1	"		"
		5200														1	Feb.	24	12·20
			5266								5645					2	Dec.	23	12
			5267	5308												2	"		20
												5797	5963			2	Jan.	8	8·30
			5265		5363											2	"	10	23·5
	5178														6259	2	"	12	23
	5168														6333	2	"		"
5135									5508							2	Mar.	1	19·25
					5347		5422			5573						3	Dec.	23	12
						5381		5484	5534							3	"	29	20·5

Ba(2)	C(1)	Mg(2)	Fe(3)	?	Tl(1)	?	Mn(2)	Pb(1)	Ba(1)	Mn(1)	Pb(2)	Fe(1)	?	Fe(2)	Lime-rick Met.	Meteoric origins.			
515	516 4	521	527		535		540	546	553	558	568	579		615	634	Wave-lengths of probable origins.			
											5662	5753			6265	3	Jan.	10	22
		5211	5265		5325											3	„		23
		5247		5300				5476		5560						4	Dec.	23	19·40
		5247			5343	5387		5466								4	„		20·45
					5367	5387	5416	5453								4	„		20·55
			5288		5349				5514	5560						4	„	25	12·30
								5490	5544	5570	5683					4	Jan.	6	23·4
											5662	5775		6120	6287	4	„	10	22·19
											5662	5753		6120	6333	4	„		„
											5647	5770		6120	6356	4	„		„
		5206	5296		5357				5505							4	Feb.	24	12·20
		5232	5296			5381			5516							4	„		„
		5221	5296			5370			5505							4	„		12·30

There are only six lines for which no origins can at present be suggested. The discrepancies between the readings of the same lines at different times are so great that a few outstanding lines are only to be expected.

It now remains for future observers to determine by direct comparisons whether the coincidences suggested are real, or merely accidental approximations.

VII. *The Norwegian Observations.*

The Report of the Norwegian Polar Station at Bossekop in Alten, in connexion with the International Polar Investigation (1882–83), gives the results of a few interesting observations of the aurora spectrum. Herr Krafft states that in general only the characteristic aurora line (558) is seen, even in strong auroræ. The red line occasionally appears very conspicuously, but only in flashes.

The wave-lengths obtained for the aurora line were 5595, 5586, and 5587. Unlike most observations, these place the aurora line on the less refrangible side of the manganese fluting. Hence, we have an additional reason for neglecting the difference between the wave-length of the brightest edge of the manganese fluting, and the commonly accepted wave-length of the aurora line, as given by Angström.

On account of the rapid flashing-up and disappearance of the red line only one measurement could be made, and the wave-length obtained was 6205. If this reading be reduced in the same proportion as those of the green line, a wave-length is obtained which agrees almost perfectly with that of the brightest edge of the iron fluting.*

These observations are the latest which have been published, and were obviously made with a full knowledge of all previous work, so that their importance must be strongly insisted upon.

It is fair to assume that the red line is due to iron, because we know that the effect of a slight increase in the intensity of the discharge which produces an aurora in which only the manganese fluting is visible would be to bring out the iron vapour. Hence in an aurora in which the green line is constant, and the red line is only intermittently visible, there must be a discharge in which there are sudden fluctuations in intensity, and a simple cause of the reddening of the aurora is now before us.

VIII. *The Spectrum of Lightning.*

If the origin of the auroral spectrum is really that which I have assigned to it, in lightning in which the electric action is feeble we

* These observations were not available to me before the preceding maps were made, so that the iron fluting has been omitted from them.

ought to again meet with some of the lines indicating higher temperatures.

Dr. Schuster made a series of observations on the spectrum of lightning in Colorado in 1878. The region of the spectrum dealt with extended from wave-length 500 to 580, and the following lines were observed:—

559·2
533·4
518·2
516·0

There can be little doubt that the first line on the list is the remnant of the manganese fluting at 558, the same as seen in auroræ. The second is in all probability the thallium line at wave-length 535, the third is probably *b* (518·3), and the fourth the edge of the carbon fluting at 516.

The lines at 559·2 and 516 were only seen on one occasion.

These observations are of very great importance, inasmuch as they appear to indicate that the difference between the spectrum of feeble or diffused lightning and the spectrum of aurora is due to a difference of temperature only.

Not only can we thus trace the difference in the spectrum as we pass from aurora to lightning, but just as we can trace the effects of gradually increasing temperatures on the spectrum of aurora, we can trace the changes due to variations in the intensities of lightning discharges, as I shall now proceed to indicate.

The spectrum of lightning as observed by Schuster in Colorado was obviously one produced by a comparatively feeble discharge. It differs from what may be conveniently called a "high-temperature aurora" only in having Mg 500 replaced by *b*. It is important to note, however, that the difference in the number of lines often seen in auroræ and in lightning is in all probability due to the fleeting character of the latter.

As we pass to the spectrum of such a discharge as Vogel observed in September, 1871, the 500 line of nitrogen makes its appearance, and Mn 558 disappears. Vogel's complete list of lines* is as follows:—

534·1
518·4
500·2
486·0
$\left.\begin{array}{r}467{\cdot}3\\ \text{to } 458{\cdot}3\end{array}\right\}$ broad band.

The band seen by Vogel was in all probability the carbon band

* Poggendorff's 'Annalen,' vol. 143, p. 654.

which is seen in the "bright-line stars," and it appears to be the most visible of the carbon bands from the same reason in both cases, namely, the absence of continuous spectrum in the blue.

The last stage in the spectrum of lightning seems to be that in which the brightest lines in the spectrum consist entirely of lines of nitrogen. Such a spectrum has been observed by Col. John Herschel, the following lines being recorded:—

569·7
500·9
463·6

These are the three strongest lines of nitrogen, the wave-lengths of which, according to Thalén, are—

$$\left.\begin{matrix}567{\cdot}8\\566{\cdot}6\end{matrix}\right\}\text{double line.}$$
$$\left.\begin{matrix}500{\cdot}5\\500{\cdot}2\end{matrix}\right\}\quad\text{"}\qquad\text{"}$$
463·1

We have, therefore, an almost complete sequence of electrical discharges through our atmosphere, from discharges so feeble that we only see the 500 fluting of magnesium, or the 1st fluting of manganese in their spectra, to those in which the brightest lines of nitrogen, characteristic of intense discharges, are the brightest lines visible. It is important to note that in the latter case we have to deal with discharges through the lower and denser portions of the atmosphere. The conditions of the two extreme cases are therefore very different, and the spectra differ accordingly. In one case the discharges pass through rarefied air charged with meteoric dust, whilst in the other they pass through dense air which is comparatively free from such dust.

In experiments with large air vacuum-tubes the *lines* of nitrogen are never seen, and it is extremely improbable, therefore, that they would occur in weak discharges through a space which is much less confined. Hence, when the line at 500 is seen in conjunction with the fluting of manganese, it is in all probability due to magnesium and not to nitrogen.

The forked lightning discharge can be imitated by a jar spark, or by the spark from an electrical machine, and the brightest lines in the spectra, as we have seen, are identical.

Fig. 20 shows the various spectra of air charged with meteoric dust when illuminated by electrical discharges of gradually increasing intensities. The lowest temperature of all gives the Mn fluting at 558. With the first increase in intensity the iron fluting (615) is at times momentarily added, then magnesium, lead, thallium, and carbon

FIG. 20.—Map showing the sequence of Spectra in electrical discharges of gradually increasing intensities through the atmosphere, the feebler discharges taking place in the rarefied regions impregnated with meteoric dust. (The thick white horizontal line indicates that no observations were made in that region.)

until there is a complete spectrum. The next stage of increasing intensity is that observed by Schuster in which magnesium is represented by *b*. Then comes Vogel's spectrum, entirely without manganese, but with *b*, Tl (535), H (F, 486), C band (468—474), and N(500). Schuster did not make observations beyond 500, so that the continuity in that region is apparently broken. It is possible that the broad band in the blue observed by Vogel was the group of nitrogen lines, the brightest of which is at 463; but in that case it is difficult to understand why a decided maximum was not recorded. Finally, we have the spectrum observed by Col. Herschel, in which those nitrogen lines appear brighter than all the rest, exactly as they appear in an intense spark discharge in our laboratories.

The question will probably arise in some minds how it is that if we assume that the luminosity of nebulæ and auroræ both proceed from meteoric dust, that in the case of the nebulæ we have to deal chiefly with the magnesium fluting at 500, whereas in the case of the auroræ the line most constantly seen by itself is the manganese line at 558? The importance of this question becomes evident when we remember that the line 558 is seen for hours without the interference of any other line whatever, and seen under conditions which indicate that the higher reaches of the atmosphere are so full of the glowing stuff which produces the line that the light is sufficiently intense to be reflected by the particles lower down. It may be that in this difference we have an important piece of evidence regarding the origin of the luminosity in the two cases in question.

In the case of the nebulæ, the light of which I have attributed to collisions, it is obvious that the collisions which produce the lowest temperature will always be greatest in number, that is to say, there will be more grazes than smashes. In any case, however, where the luminosity is produced in this way there will be sufficient temperature brought about by impacts to volatilise the constituents of the meteorites. Considering the meteorites merely from what we know about their composition from those which have fallen on the earth, we must assume that the largest constituent of meteorites is olivine.

Where, therefore, we are dealing with collisions merely, we should expect to get the spectrum of olivine produced say 10,000 times, while the spectrum of the other substances would only be produced once in consequence of more extensive collisions. But when we pass from the nebulæ to the meteoric dust in our air we are no longer dealing with collisions; we are dealing with luminosity brought about by electrical discharges; and it requires no long argument to show that these electric discharges would be more likely to travel along and to render luminous the metallic constituents of the dust rather than the silicates of magnesium or of any other metal.

In this way, then, we should expect to get electrically exhibited the

spectrum of the substances in the iron dust which came out under the lowest conditions of electrical excitation. I have previously shown that under these circumstances what we do get is invariably the spectrum of manganese with its first fluting at 558, and that long before the spectrum of iron itself is seen.

Should this line of argument be accepted, we have in it an additional proof of the suggestion that the luminosity of nebulæ is really due in great part to collisions, not to electrical excitation of any kind in the first instance.

I think it will be granted after what has preceded, that there is strong evidence of an intimate relation between the spectrum of the aurora and the spectra of meteorites and meteor-swarms. Certainly the coincidence is such as to justify us in regarding meteoric dust as the origin of the spectrum until a better and more probable origin is demonstrated.

How this view will meet the periodicity and geographical distribution of auroræ remains to be investigated; the question may be asked whether the earth sometimes meets greater quantities of aurora-producing matter revolving round the sun than at other times, and whether in this way the periodicity may be explained.

IX. *The Aurora and the Zodiacal Light.*

Since the shooting star ignition level lies between 75 and 50 miles in height, and auroræ have been seen at heights of over 100 miles, it seems probable that the matter which reaches the earth from space is in the main of three degrees of fineness, and gives evidences of its existence at three different heights, the finest furnishing materials for auroral displays at heights reaching to 130 miles,* the mean finenesses igniting at a height of 75 miles, and giving rise to the appearance of falling stars, till a height of 50 miles is reached, when it is all consumed; and the coarsest of all, which at times reach the surface itself as meteoric irons or stones.

An additional argument in favour of the meteoric theory of the

* Capron and Herschel, "On the Auroral Beam of November 17, 1882" ('Phil. Mag.,' May, 1883). Professor Herschel, from measurements made 1863–67, determined the height of long white stationary auroral arches to be close upon 100 miles.

Herr Sophus Tromholt ('Nature,' vol. 27, p. 394) gives 90 miles, and Baron Nordenskiöld ('Scientific Work of the "Vega" Expedition,' Part I, p. 401–450), gives 115 miles.

Professor Herschel has also referred to measurements of auroral arches by Dr. Dalton ('Phil. Trans.,' 1828, p. 291), who found 100 miles. Professor Potter's determinations ('Cambridge Phil. Trans.,' 1845) of the heights of auroral arches observed in September and October, 1833, ranged, on the other hand, between 55 and 85 miles.

aurora is furnished by other phenomena, which sometimes accompany them.

During the great aurora of January, 1831 (Poggendorff's 'Annalen' of that year), a bright yellow streak was seen to rise with common cloud velocity, forming an arch from west to east, becoming invisible in the west by the time it had reached the east.

During the same aurora Professor Bischoff, at Burgbrohl, saw a moving cloud, as bright as the Milky Way, pass from east to west in five minutes.

During another aurora, December, 1870, Professor Rudberg, of Upsala, saw a very bright patch, of double the dimensions of the moon's disk, moving with great velocity behind the auroral beams.

On November 2, 1871, Dr. Groneman saw a strange, feather-like, brilliant arch, striped parallel to its well-defined sides, and changing its curve during its visibility of two hours' duration. Dr. Vogel determined the auroral character of its spectrum.*

On May 17, 1875, Mr. Lefroy (Freemantle, Western Australia) describes a similar feather-like appearance, which he considered to be converging streams of infinitely minute particles of matter passing through space at a distance from the earth less than that of the moon, and at which the earth's aërial envelope may still have a density sufficient, by its resistance, to give to cosmic dust passing through it with planetary velocity that slight illumination which it possesses.†

On November 17, 1882, however, was seen the most remarkable display of this nature in the middle of an intense aurora then visible. Again the appearance was feather-like, again the spectrum was auroral, but the strange object moved across the sky, at a height of 133 miles, as determined by Capron and Herschel, and with a planetary velocity of between 10 and 15 miles a second!

Dr. Groneman did not hesitate at the time to look upon it as a mass of meteoric dust traversing the higher reaches of our air, and regarded it as a strong confirmation of the view which he had resuscitated,‡ a conclusion in which I concur.

The above results also strengthen the view that the aurora is very similar in some respects to the zodiacal light. Such a connexion is indicated by the fact that when we have greatest number of auroræ, in spring and autumn, the zodiacal light is also best visible. The spectroscopic observations of Ångström and Respighi show that the spectrum of the zodiacal light consists of the characteristic line of the aurora and a short continuous spectrum, and thus furnish further evidence of the connexion suggested. The observations of Wright and others, showing that the spectrum is continuous, are not at

* 'Nature,' vol. 27, p. 297.
† 'Nature,' vol. 12, p. 330.
‡ 'Nature,' vol. 27, p. 296.

variance with Ångström's observation, for we should expect the spectrum to be somewhat variable.* It is probable that the observations showing nothing but continuous spectrum were made when the temperature was only sufficient to render the meteoric particles red hot. That the zodiacal light does consist of solid particles, or at all events of particles capable of reflecting light, is shown by the polariscope.

No one has ever gone so far as to suggest that the zodiacal light is an atmospheric phenomenon, and yet the principal line in its spectrum is identical with that in the spectrum of the aurora. We have, therefore, an additional reason, if one be required, for discarding any atmospheric origin which has been suggested for the auroral spectrum.

Part II.—Fallen Dust.

We have now complete evidence of the existence of meteoric dust in the atmosphere, first, from the known number of meteorites which enter the atmosphere, and secondly, from the spectroscopic observations of auroræ. This dust will finally reach the earth's surface, and it is exceedingly interesting to trace its subsequent history as far as possible.

The detection of such dust which falls on the general surface of the earth is almost hopeless, but that which falls on the sea will have a chance of accumulating where the water is quietest. The researches of Messrs. Murray and Renard† during the "Challenger" Expedition seem to indicate that such an accumulation really takes place.

An examination of the deep-sea deposits collected during the expedition has led them to believe that certain small "magnetic spherules" are totally unlike particles of iron derived from basaltic rocks or from furnaces, and that their origin is probably meteoritic. In addition to these, great numbers of the so-called "manganese nodules" were found in the red muds from deep-sea bottoms. Messrs. Murray and Renard incline to the belief that these owe their origin chiefly to the decomposition of volcanic rocks, but my own researches seem to show that they may be at least partly formed by the accumulation of altered meteoric dust.

An analysis of one of these nodules by Professor Renard ('"Challenger" Report, Narrative,' vol. 1, Part II, p. 1048), gives the following:—

* Since the above was written I have received a letter from Mr. T. Sherman, stating that he has reason to believe that the appearance of the 558 line in the zodiacal light has a regular period.

† "On the Microscopic Characters of Volcanic Ashes and Cosmic Dust, and their Distribution in Deep-sea Deposits," 'Edinb. Roy. Soc. Proc.' and 'Nature,' vol. 29, p. 585.

Water (H_2O)	9·51
Silica (SiO_2)	19·34
Lime (CaO)	3·19
Alumina (Al_2O_3)	6·36
Ferric oxide (Fe_2O_3)	26·70
Magnesia (MgO)	1·79
Oxide of manganese (MnO)	26·46
„ nickel (NiO)	1·82
Oxygen	6·31
	101·48

The specimen examined was from Station 276, 2350 fathoms, South Pacific.

I have observed the spectra of some of the nodules, which were kindly placed at my disposal by Mr. Murray.

In the oxy-coal-gas flame, lines of Na, Tl, Li, K, Mn, and Fe are seen. The brightness and persistence of the thallium line at 535 is very remarkable, and is especially interesting since the line is seen in the aurora and in one or two meteorites. The red line of lithium, which is seen in many of the meteoric flames, is also bright in the spectrum of the nodules. The manganese fluting at 558, the one coincident with the chief line of the aurora, is also seen in the spectrum of the nodules, but it is not nearly so bright as the thallium line. The iron lines are very faint. As might be expected, from the association with sea-water, the lines of sodium and potassium are very bright. A photograph of the flame spectrum shows lines of manganese, and some of the strongest violet lines of iron.

When some fragments of the nodules are placed along an end-on vacuum-tube and the spark passed, flutings of carbon and lines of hydrogen appear, almost exactly as they do when meteorites are subjected to the same treatment. When the tube is made red hot, the thallium line becomes very bright, and also the yellow and green lines of sodium.

It will be seen that the spectra of the nodules are somewhat different from those of meteorites, chiefly in the relative intensities of the lines, but the difference can probably be explained by considering the effect of sea-water. I have the authority of my friend Professor Thorpe for stating that thallium and manganese would be the most likely of the meteoric constituents to form insoluble compounds, and hence these are what we should expect to find in deep-sea accumulations of meteoric dust. The spectroscopic observations therefore seem to show that it is not improbable that the manganese nodules owe their origin, in some part at least, to meteoric dust.

At the suggestion of Professor Renard I separated some of the iron spherules from the nodules by dissolving in dilute hydrochloric acid,

and passing a magnet through the insoluble residue. In the oxy-coal-gas flame the spectrum of the spherules consisted of lines of iron, sodium, and potassium, and the flutings of manganese, but there was absolutely no trace of thallium. The other portion of the residue, however, gave the thallium line as bright as the nodules themselves. The solution, when evaporated to dryness, gave no indications of thallium.

If we are justified in regarding the partly meteoric origin of the nodules as established, the excess of thallium shows that each nodule represents a very considerable quantity of meteoric dust, since there is only a comparatively small proportion of thallium in meteorites. This further suggests that an enormous quantity of meteoric dust passes through our atmosphere, especially as that which falls on the sea only represents a portion of the total amount.

III. "SUGGESTIONS ON THE ORIGIN OF BINARY AND MULTIPLE SYSTEMS."

In connexion with the explanation of the variability of the bodies of Group II, which I suggested in the Bakerian Lecture for the last year, I indicated that in the absence of spectroscopic details, the colours of the components of double stars might enable us to determine whether both have condensed from double or multiple nebulæ, or whether the companions are later additions to the systems. I also referred to some difficulties in the discussion.

On further consideration some of the difficulties have disappeared, and I now propose to return to the subject, limiting myself for greater simplicity to binary systems.

For this purpose it is necessary to begin by stating briefly what we know relating to the colours of the different groups of celestial bodies, adopting the classification which I suggested in the Bakerian Lecture.

I. *Colour Phenomena.*

As far as we at present know, the colours associated with the different groups of celestial bodies are in all probability as follows:—

Group I......	Blue, greenish blue, white, or pale grey.
Group II	Yellowish red.
Group III......	Yellow to greenish white.
Group IV......	Bluish white.
Group V......	Greenish white to yellow.
Group VI......	Reddish yellow to blood red.
Group VII......	Dark or nearly dark bodies.

The blue colour of some of the more advanced members of Group I, which are all faint, is probably due to the bright blue fluting of carbon which stands out beyond the end of the continuous spectrum. They are really blue, and not apparently so because of any absorption of the red. That in the case of double stars this colour is not due to optical causes or complementary colours is shown by the fact that there are some equally faint stars which are seen to be red under similar contrast, and instrumental, conditions.

Pechüle has observed the spectrum of one faint blue star, and his observation bears out my view of their nature. He says:— ". . . . . . 15′ au Nord de cette étoile je trouve une étoile de $7^{m \cdot}$, qui a un spectre très singulier ni du III ni du IV type. La partie moins réfrangible du spectre n'est qu'indistinctement coupée et un peu plus lumineuse du côté du rouge. Après un large intervalle noir vient une zone étroite d'un éclat tout-à-fait prédominant qui s'éteint rapidement du côté plus réfrangible, et forme la fin du spectre. La couleur de l'étoile est bleuâtre." (Pechüle, 'Expédition Danoise,' 1882, p. 40.)

The green colour of the unadvanced members of Group I is probably due to the magnesium radiation; thus, the Ring Nebula in Lyra is green, and we find that its radiation consists almost entirely of the magnesium fluting at wave-length 500. The bodies in the same group which are white, or pale grey, in all probability add the radiation of carbon and incandescent meteorites to the foregoing. How far spectroscopic observations made with the assistance of large telescopes will confirm these views or prove them to be erroneous remains to be seen; for the present, however, we may take the colours associated with bodies in Group I as I have stated them.

The colours which I have associated with Groups II and VI are those given by Dunér.

The prevailing tints in bodies of Groups III and V are white, yellow and orange, so that when we see a yellow star we cannot say from colour alone what group it belongs to.

The later species of Group III will be white and greenish white, the latter being the most advanced. With a further increase of temperature, stars of Group IV are formed, the colour becoming bluish white owing to the increase of blue light. After this the temperature begins to fall. The first species of Group V will also be greenish white on account of the reduction of blue light, and the next species will be white. After this, the various species of the group will vary from yellowish white to orange.

The stars of Group IV, α Lyræ, and Sirius being the most brilliant types, are bluish white.

The bodies of Group VII have little or no inherent luminosity.

II. *General Statement of Conditions.*

In discussing the question whether the components of a binary star have condensed from the same nebulosity or not, a difficulty arises on account of the fact that, according to my theory of their constitution, there will be no constant relation between the mass of a swarm and its brightness. When we see a "star" of a certain magnitude, we cannot tell from its brightness alone whether it is a large faint one or a small bright one; for a large body at a low temperature may be equalled, or even excelled in brightness, by a smaller body at a higher temperature. But when we know the spectra of the bodies, we also know their relative temperatures. In the absence of spectroscopic details, colour helps us to a certain extent, as I have shown.

If a pair of stars of unequal masses have condensed from a double nebula, the smaller one will be further advanced along the temperature curve than the larger one; the colours and spectra will be different, *but it is not imperative that the magnitudes shall be unequal.* The smaller swarm, because it must be in more rapid movement round the common centre of gravity, will suffer more quasi-tidal action and therefore collisions per unit volume; it will therefore condense more rapidly than the larger one; it will soon become as luminous, and afterwards will for a time be considerably hotter than the larger one.

If the masses be very unequal, the smaller one will have the smaller magnitude for a longer time. When there is a great difference in magnitude, therefore, it is fair to assume that the one with the smaller magnitude has also the smaller mass.

Another difficulty in the discussion, in the absence of spectroscopic details, is due to the similarity in colour of bodies at equal heights on the opposite sides of the temperature curve. Thus, as already stated, bodies in Group III have, as far as we at present know, exactly the same colour, namely, yellow, as those in Group V. Again, many of the members of Group II have the same colour as some in Group VI.

The general conditions with regard to this subject may be thus briefly stated:—If the *magnitudes*, colour, and spectra of the two components of a physical double are identical, both had their origin in the same nebulosity with two condensations, or in a double nebula.

If the *magnitudes are nearly equal*, but the colours and spectra different, it may be that the one with the most advanced spectrum has the smaller mass, and if the advance is in due proportion, we are justified in regarding them as having had a common origin.

If the *magnitudes are very unequal*, we may take the one with the smaller magnitude as having the smaller mass, and if it is proportionately in advance, as indicated by its spectrum, or colour, we may

regard both components as having had a common origin. If the smaller one be less advanced than the larger one, we have to regard it as a late addition to the system.

If the two stars are of equal mass and revolve round their common centre of gravity they have in all probability done so from the nebulous stage, and therefore they will have arrived at the same stage along the evolution road, and their colours and spectra will be identical.

If, however, the masses are very different, then the smaller mass will run through its changes at a much greater rate than the larger one. In this way it is possible that the stars seen so frequently associated with globular nebulæ may be explained; while the nebula with a larger mass remains still in the nebulous condition, the smaller one may be advanced to any point, and may indeed even be totally invisible (Group VII), while the parent nebula is still a nebula. This condition may be stated most generally by pointing to those double stars in which the companions are small and red, although we know nothing for certain with regard to their masses. But if we pass to the other category in which it may be suggested that the companion is added afterwards, the most extreme form would be a nebula revolving round a completely formed star, like an enormous comet round the sun; a less extreme form would be a bright line star, or a star of the second group, revolving round one of a higher group. In this case the colour would be blue or greenish-blue or grey.

III. *Light Curves.*

I find that the best way of dealing with this question is to represent the life of each component by a curve, in which the ordinates represent time and the "magnitude" of the star. Then, if the colours and magnitudes are consistent with the curves beginning at the same point, we are justified in regarding both as having condensed from the same nebulosity. If not, in all probability the companion would be a later addition.

The form of the light curve, which represents the effect of increase and decrease of temperature, will probably be something like fig. 21. We should expect the curve to be somewhat similar to the light curves of the regular variables of Group II, where the increase in luminosity is due to the collision of two meteor-swarms. Here there is a rapid rise to maximum, and a steadier fall to minimum. This is confirmed by the fact that there is apparently a greater number of stars of Group V than of Group III, though on this point I cannot yet speak with any certainty. If this should turn out to be so, the fact would appear to indicate that the time of existence of a body as a star of Group V is probably longer than the time during which it exists as a condensed meteor-swarm under the conditions of Group III. During

FIG. 21.—*Light curve of a meteor-swarm during the various stages of condensation.* The numbers represent the spectroscopic groups, I being the least condensed, and VII the most condensed.

its existence as an uncondensed swarm, however, the increase of luminosity of the swarm will be very steady; hence there will first of all be a gradual increase of luminosity; this will be followed by a rapid rise to maximum, and afterwards a steady fall, until finally all luminosity disappears.

The light curves being of this form, if we begin with two uncondensed swarms of equal masses and conditions, the curve for each will be the same in length and in the point of maximum luminosity. It will be a neck and neck race, and we shall have equal brilliancy, similar colour and spectrum throughout. Such stars I call Class I.

IV. *Binary Stars, Class* 1.—*Equal Magnitudes and Similar Colours* (*not Yellow*).

The first question is: Are there any such stars, for from the existence of so many nearly equal double nebulæ in the heavens we should expect a large number.

For the purpose of this inquiry I have used the Bedford Catalogue,* and have limited myself to the stars which afford the strongest evidence of being binary systems. In the absence of any spectroscopic survey of such systems, I am forced to content myself with similar or nearly similar colours.

The following is a list of the binary stars given by Smyth, in which the magnitudes and colours of the components are almost identical. I except for the present those in which both components are yellow for a reason before stated.

In these cases the two curves representing the lives of the components will be identical, or nearly so, and will be as in fig. 21. One of the components may have a somewhat smaller mass, and, therefore, a shorter time of existence, as a self-luminous body, than the other, but the magnitudes and colours may still be nearly equal, or suffi-

* 'A Cycle of Celestial Objects,' Smyth and Chambers; 2nd edition, 1881.

ciently so for my present purpose in the present state of our knowledge:—

Table I.—Binary Stars, Class 1.

Smyth's No.	Name.	Magnitudes.		Colours.	
18	38 Piscium.............	7½	8	Light yellow ..	Flushed white.
40	181 P. O. Cassiopeiæ.....	8	8½	Flushed white..	White.
85	123 Piscium......	6½	8½	Yellowish	Pale white.
108	209 P. I. Piscium	7	7½	White	White.
*117	α Piscium	5	6	White	White.
128	259 B Andromedæ.......	7	8	White	White.
170	ε Arietis	5	6½	Pale yellow	White.
201	7 Tauri	6	6½	White	Pale yellow.
387	32 Orionis.............	5	7	Bright white ...	Pale white.
440	301 P. VI Lyncis........	6	6½	White	White.
480	1104 ε Puppis..........	7	9	White	White.
484	α Geminorum	3	3½	Bright white ...	Pale white.
492	170 P. VII Canis Minoris	7	8	White	Ash-coloured.
562	ι Cancri............ ...	5½	7	White	Yellow.
586	157 B. Lyncis...........	7½	8	White	White.
681	229 P. X Leonis	8	8	White	White.
698	ξ Ursæ Majoris.........	4	5½	Subdued white .	Greyish white.
779	1606 Σ Can. Venatico.....	6½	7½	White	White.
851	γ Virginis...............	4	4	Silvery white...	Pale yellow.
860	1678 Σ Virginis	6½	7½	Very white	Yellow white.
915	127 P. XIII Virginis	8	9	Pale white	Yellowish.
946	238 P XIII Virginis	7	8½	White	White.
961	R. Bootis...............	7½	9	White	White.
986	ζ Bootis................	3½	4½	Bright white...	Bright white.
1007	44 Bootis...............	5	6	Pale white.....	Lucid grey.
1026	1 B. Coronæ Borealis	6	6½	Very white	Very white.
1031	η Coronæ Borealis.......	6	6½	White	Golden yellow
1035	μ^2 Bootis...............	8	8½	Greenish white.	Greenish white.
1077	49 Serpentis	7	7½	Pale white.....	Yellowish.
1130	2106 Σ Ophiuchi........	7	9	White	White.
1150	μ Draconis..............	4	4½	White	White.
1213	τ Ophiuchi	5	6	Pale white.....	Pale white.
1227	73 Ophiuchi	6	7½	Silvery white...	Pale white.
1274	ε Lyræ	5	5½	White	White.
1326	108 P. XIX Draconis	8	9	White	White.
1442	λ Cygni.................	5	6	Bluish	Bluish.
1457	2744 ε Aquarii..........	6½	7½	White	White.
1490	29 B. Pegasi............	7½	8	White	White.
1515	ξ Cephei................	5	7	Bluish	Bluish.
1523	148 B. Pegasi...........	7	8½	White	White.
1535	ζ Aquarii...............	4	4½	Very white	White.
1536	37 Pegasi...............	6	7½	White	White.
1552	219 P. XXII Aquarii	7½	8	Yellow........	Flushed white.
1573	69 P. XXIII Aquarii	8	8½	Flushed	Flushed.

V. *Binary Stars, Class 2.—Equal Magnitudes and Similar Colours (Yellow).*

The following list contains those binary stars in which both com-

* These colours are as given by Dawes.

ponents are yellow and of nearly equal magnitudes. If both components shall be found to have identical spectra, thus placing them in the same group, a point which their colour leaves indeterminate, their "life curves" will be coincident. If one is found to belong to Group III, however, and the other to Group V, they can still be represented by two curves beginning at the same point, but with the ascending side of one intersecting the descending side of the other as in fig. 22. The places occupied by the stars are indicated by dots; the portions of the curves to the left of the dots represent the stages already passed through, those to the right the stages still to be gone through. This also applies to the diagrams which follow. In the

FIG. 22.—The light curves of the two components of a binary star, in which both components are yellow, and of equal or nearly equal magnitudes.

former case the masses of the two components would evidently be equal, or nearly so, while in the latter case, one would be considerably larger than the other. Hence, in all cases where the components are yellow and of nearly equal magnitudes, we are justified in regarding them as having possibly condensed from the same nebulosity.

Table II.—Binary Stars, Class 2.

Smyth's No.	Name.	Magnitudes.		Colours.	
8	316 B. Cephei	6½	7	Yellow........	Deeper yellow.
12	318 B. Cephei	7	7½	Yellow........	Yellowish white.
46	36 Andromedæ..........	6	7	Bright orange..	Yellow.
487	149 P. III Puppis	6	6	Topaz tinted...	Topaz tinted.
524	ζ Cancri.................	6	7	Yellow........	Orange tint.
689	9 P. XI Leonis..........	7½	7½	Faint yellow...	Faint yellow.
690	1516 Σ Draconis.........	7½	8	Yellowish	Ashy yellow.
895	42 Comæ Berenicis	4½	5	Pale yellow....	Pale yellow.
981	α Centauri.............	1	2	Yellow........	Yellow.
1463	61 Cygni	5½	6	Yellow........	Yellow.
1483	20 B. Pegasi............	7	7	Yellowish	Yellowish.
1598	37 B. Andromedæ.......	6	6	Yellowish ...	Yellowish.

VI. *Binary Stars, Class 3.—Equal or Nearly Equal Magnitudes, one Star being Blue.*

There is a considerable number of binary stars in which the magnitudes of the components do not differ very much, but where one star is blue. If we take these blue stars as belonging to Group I we shall have an average case represented by fig. 23, both curves starting at the same point. From this point of view the companion which has the smaller magnitude has the greater mass, and the system is young.

FIG. 23.—Light curves of the components of a binary star of Class 3, in which both components have equal or nearly equal magnitudes, one being blue.

If these curves are a fair representation of binary stars of this class, it is clear that we ought to find the primaries in every case, white with a tendency to yellow. This is a severe test, but yet on referring to the following table, which is a list of such binary stars, it will be seen that there is not a single case in which the primary is not white or yellow:—

Table III.—Binary Stars, Class 3.

Smyth's No.	Name.	Magnitudes.		Colours.	
1	I. Cassiopeiæ..............	7	8	Yellowish white	Bluish.
21	49 Piscium..............	7	10½	White	Blue.
24	51 Piscium..............	6½	9	Pearl white....	Lilac.
50	251 Piscium..............	8	9	Pale orange....	Clear blue.
63	ζ Piscium..............	6	8	White........	Pale grey.
120	10 Arietis..............	6½	8½	Yellow........	Pale grey.
150	33 Arietis..............	6½	8½	Pale topaz....	Light blue.
202	98 Eridani..............	6½	9	Yellow........	Plum colour
438	14 Lyncis..............	5½	7	Golden yellow.	Purple.
442	38 Geminorum..............	5½	8	Light yellow..	Purple.
507	5 Puppis..............	7½	9	Pale yellow...	Light blue.
596	ω Leonis..............	6½	7½	Pale yellow...	Greenish.
671	54 Leonis..............	4½	7	White.......	Grey.
706	ι Leonis..............	4	7½	Pale yellow...	Light blue.
722	17 Crateris..............	5½	7	Lucid white...	Violet tint.

Table III—*continued.*

Smyth's No.	Name.	Magnitudes		Colours	
922	25 Canum Venaticorum ..	6	8	White	Blue.
939	1785 Σ Bootis..........	7¼	8	White	Bluish.
971	70 P XIV Libræ	7½	9¼	Pale yellow	Greenish
995	ξ Bootis	3½	6¼	Orange........	Purple.
1081	σ Coronæ Borealis	6	6½	Creamy white .	Smalt blue
1101	λ Ophiuchi.............	4	6	Yellow white...	Smalt blue
1132	167 B. Herculis.........	7	8¼	Yellowish	Bluish.
1219	70 Ophiuchi............	4½	7	Pale topaz	Violet.
1229	417 B. Herculis	6	7½	Yellow........	Bluish.
1444	4 Aquarii...............	6	8	Pale yellow....	Purple.
1498	μ Cygni................	5	6	White	Blue
1522	41 Aquarii	6	8½	Topaz yellow ..	Cerulean blue.
1524	33 P. XXII Pegasi......	7½	10½	Lucid yellow...	Sea green.
1528	γ Aquarii	4	14	Greenish tinge .	Purple.
1531	33 Pegasi...............	6½	10	Yellowish	Blue.
1586	107 Aquarii	6	7½	Bright white ...	Blue.

VII. *Binary Stars, Class 4.—Very Unequal Magnitudes, the smaller Star being Blue.*

The next class to be considered is that in which the companion is of relatively small magnitude, and is blue, green, or grey, the primary usually being white or yellow.

A binary star of this class can be equally well explained by starting the two curves at the same point, or starting one later than the other. In the former case we should have to regard the one with the smaller magnitude as having the greater mass, and the two curves would be as in fig. 24, *a*. If we take the one with the smaller magnitude as having the smaller mass we shall have the curves as in fig. 24, *b*.

FIG. 24.—Light curves of the components of a binary star of Class 4. *a* represents the case on the assumption that both components condensed from a double nebula, whilst *b* represents the case on the assumption that the companion is a cometary addition.

It seems probable, therefore, that we shall never be able to tell whether the components of a binary star of this class have both con-

densed from the same nebulosity or not; but since the components of the majority of binary stars appear so far to have had in all probability a common origin, there is no reason why we should rather regard these as having had a different one. The following is a list of them taken from Smyth's 'Celestial Cycle':—

Table IV.—Binary Stars, Class 4.

Smyth's No.	Name.	Magnitudes		Colours	
2	α Andromedæ	2	11	White	Purplish
9	γ Pegasi	2½	11	White	Pale blue.
14	ι Ceti	4	11	Bright yellow	Deep blue.
16	42 Piscium	7	13	Topaz yellow	Emerald green.
48	μ Andromedæ	4	16	Bright white	Dusky grey.
83	40 Cassiopeæ	6	11	Yellow	Pale blue
118	ι Trianguli	5½	15	Bright yellow	Dusky.
340	δ Orionis	2	7	White	Pale violet.
440	59 Aurigæ	6	11	Pale yellow	Livid.
463	δ Geminorum	3½	9	Pale white	Purple.
533	67 P. VIII Cancri	6	13	Pearl white	Violet.
551	δ Cancri	4½	12	Straw coloured	Blue.
554	ε Hydræ	4	8½	Pale yellow	Purple.
565	ι Ursæ Majoris	3½	11	Topaz yellow	Purple.
569	σ² Ursæ Majoris	5½	9½	Flushed white	Sapphire blue.
926	1 Bootis	6	10	Sapphire blue	Smalt blue
1354	δ Cygni	3½	9	Pale yellow	Sea green.
1499	κ Pegasi	4	15	Pale white	Purple.
1550	ξ Pegasi	5	15	Pale yellow	Blue.
1567	π Cephei	5	10	Deep yellow	Purple.

VIII. *Binary Stars, Class 5.—Unequal Magnitudes, the fainter Star being Red.*

There are a few binary stars in which the companion is red. The red component has probably a smaller mass than the primary, and is consequently, further advanced along the temperature curve. Fig. 25

FIG. 25.—Light curves of the components of a binary star of Class 5, in which the companion is red and relatively small.

represents an average case of such a binary star; both curves starting at the same point. In this case, it will be seen that the companion has almost run through all its stages, whilst the primary has still several stages to pass through. This may be regarded as a more advanced stage of binary stars of Class 2.

We have here again a severe test, for if these curves represent anything like the truth, the primaries ought in every case to be greenish white, white or yellow. On referring to the list it will be seen that this condition is satisfied in every case. To make quite sure that δ Herculis belonged to this class of binaries, a special examination of its spectrum was made at Kensington. This showed it to be almost as far advanced along the temperature curve as Sirius.

Only a small number of such binaries has been recorded. They are as follow:—

Table V.—Binary Stars, Class 5.

Smyth's No.	Name.	Magnitudes.		Colours.	
42	η Cassiopeiæ	4	7½	Yellow	Red.
1157	δ Herculis..............	4	8½	Greenish white.	Grape red.
1274	ε Lyræ.................	5	6½	Yellow	Ruddy.
1297	287 P. XVIII Draconis ..	7	8	White	Pale red.
1551	τ Aquarii..............	6	9½	White	Pale garnet.

IX. *Outstanding Cases*

Out of all the binary stars of which there is any record in Smyth's 'Celestial Cycle,' there are only eight which cannot be included in any of the five classes which have been dealt with. Five of these are totally indeterminate on account of the absence of a statement of the colours; they are as follow:—

Smyth's No.	Name.	Magnitudes.		Colours.	
22	λ Cassiopeiæ	6	6½	Colours	not stated.
491	α Canis Minoris	1½	..	Yellowish white	..
872	35 Comæ Berenicis	5	..	Pale yellow	Indistinct.
1053	γ Coronæ Borealis	6	..	Flushed white..	Uncertain.
1303	γ Coronæ Australis	6	6	..	..

The remaining three are as follow:—

460	λ Geminorum...........	4½	11	Brilliant white.	Yellowish.
635	γ Leonis................	2	4	Bright orange..	Greenish yellow.
1114	ζ Herculis............		6	Yellowish white	Orange.

In the first of these, λ Geminorum, the companion has probably been added since the primary condensed, for we cannot place the two components on curves which begin at the same point.

With regard to γ Leonis, there is a difficulty as to what spectrum should be associated with the greenish-yellow component, so for the present it cannot be stated whether both have condensed from the same nebulosity or not.

We cannot include ζ Herculis in Class 2, because the difference between the magnitudes of the two components is too great, but we can represent the case by starting the companion curve a little later than the primary curve. We may therefore conclude that we have here to deal with an added companion.

X. *Conclusion.*

From the foregoing lists and discussions it will be seen that in nearly all cases the components of a binary can be shown with much probability to have had their origin in double nebulæ. There are exceedingly few cases in which it seems at all likely that the companion is an addition of a cometary nature, and it is possible that even these few exceptions may be due to errors of observation.

This, then, strengthens the view that in the case of regular variable stars of Group II we are in presence of the formation of a double star, at an early period in its history when the two swarms are at times, so to speak, in contact. When the variability is not regular we are in presence of the formation of a multiple system.

I cannot omit to point out how very admirable the colour observations must have been to stand the strain to which the foregoing generalisation has subjected them, and that if equal skill be now applied to observation of the spectra of these bodies, a considerable advance in our knowledge may be looked for.

In the discussion included in this paper, I have been aided by Messrs. Fowler, Gregory, Baxandall, Porter, and Coppen. Mr. Fowler made the observations of the spectrum of the large air vacuum tube, and of the spectra of manganese nodules and iron spherules. He also classified the binary stars, Mr. Coppen assisting him in preparing the tables.

Mr. Gregory has been responsible for preparing the various tables in connexion with comets and auroræ.

Messrs. Baxandall and Porter have prepared most of the maps and drawings, for the careful reproduction of which I have to thank Mr. Collings.

I wish, as before, to tender my thanks to them for the unflagging

zeal and the intelligence with which their part of the work has been performed. I must also specially thank Mr. Fowler for his collaboration in the preparation of the paper itself, and for supervising in part the work of the other assistants.

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January 17, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "A Method of detecting dissolved Chemical Compounds and their Combining Proportions." By G. GORE, F.R.S. Received November 14, 1888.

(Abstract.)

The method described and illustrated by examples in this research is an application of the "voltaic balance" to the measurement of the amount of voltaic energy of electrolytes (see 'Roy. Soc. Proc.,' vol. 44, pp. 151 and 294), and is based upon the general truth that "when substances chemically combine they lose some of their power of exciting a voltaic couple," and the amount of this power can be measured by means of the "voltaic balance." (Sketch.)

The method is briefly as follows: Oppose and balance the current from a small voltaic couple of unamalgamated zinc and platinum in a known quantity of distilled water in a small glass vessel through a sufficiently sensitive galvanometer, by that from a perfectly similar couple, and take care by occasionally heating the platinum to redness, to avoid error caused by absorption of hydrogen.

Dissolve in separate, equal, and known quantities of distilled water a series of several mixtures of the two constituents A and B of the supposed compound, in the proportions of their atomic or molecular weights, and multiples of them, some having an excess of A, and others of B. For instance, if both are monads mix them in the several proportions represented by the formulæ 5A+4B, 4A+4B, and 4A+5B; but if A is a monad and B a dyad, then use the proportions indicated by 3A+2B, 4A+2B, and 5A+2B.

Add sufficiently minute quantities in succession of one of these solutions to the water of one of the voltaic couples until the needles of the galvanometer visibly commence to move, and note the amount added. Recharge the vessel with distilled water, clean the metals, and repeat the experiment with another of the solutions; and so on until all the solutions have been tried, and the mixture has been

found of which the largest proportion is required to move the needles, that is the one which has the smallest proportion of voltaic energy, and which has its constituents chemically united in definite proportion by weight without an excess of either ingredient in a chemically free state.

The following is an example. The combining proportion is the one which gives the smallest amount of voltaic energy, and its formula is indicated by a star (*).

$K_2SO_4+KNO_3$.

	Between 1 part in			Temp.	Average. 1 part in
KNO_3............	10,333	and	11,350 parts of water	at 20° C.	10,841
$K_2SO_4+100KNO_3$	163	,,	182		172
$2K_2SO_4+5KNO_3$..	55	,,	60	18	57
$2K_2SO_4+4KNO_3$*.	50	,,	54		52
$2K_2SO_4+3KNO_3$..	58	,,	64		61
$100K_2SO_4+KNO_3$...	870	,,	975	20	922
K_2SO_4............	2,132	,,	2,396	12	2,274

The compound is represented by the molecular formula—

$K_2SO_4,2KNO_3$.

By means of a number of suitable examples of this kind, the author shows that the dissolved substances unite together in the definite proportions by weight of their ordinary chemical equivalents. The results of several experiments indicate the existence of multiple combining proportion in a feeble degree.

Evidence is given of the existence in solution of compounds represented by the formulæ KCl,Cl, —KBr,Br, and KI,I, and these results are confirmed by means of comparative colour measurements.

The question of the limit of complexity of chemical combination of substances whilst in solution together in water is experimentally investigated, and although a definite compound was formed having the formula $K_2SO_4,16KNO_3,4AmCl,2NaCl,8KCl,32LiCl$, the limit of possible complexity did not appear to be nearly reached.

With regard to the general question, does every electrolytic substance when dissolved in water unite in definite proportions by weight with every other such dissolved substance? The author states that he has examined by the foregoing method more than 180 different mixtures of such bodies, but has not found one in which definite chemical union is not more or less clearly indicated by a minimum amount of voltaic energy, coinciding with the proportions of the ordinary chemical equivalents of the substances. The mixtures he examined included all classes of these substances, viz., of elements with elements; elements with monobasic, bibasic, and tribasic acids; acids of all these classes with each other; elements with monobasic,

bibasic, tribasic, and tetrabasic salts; monobasic, bibasic, and tribasic acids with all these classes of salts, and all these salts with each other in similar great variety. And he concludes that the relation of voltaic energy to chemical combining proportion, as already stated, is a general one, and that *every electrolytic substance when dissolved in water unites chemically in definite proportions by weight with every other such dissolved body*, provided no separation of substance occurs. And that they unite to form compounds of apparently unlimited complexity.

The method may be employed to ascertain the degrees of valency of substances, the basicity of acids, &c. It may also be used to test the purity of soluble bodies, and (as previously stated) to examine the internal constitution of electrolytes; and the author is now using it for the two latter purposes. It is capable of extensive application; by it the state of union, whether chemical or of mere mechanical mixture (possibly also the relative strength of chemical union) of nearly every electrolytic substance soluble in water, alcohol, &c., with every other such substance can be detected. provided the substances do not precipitate each other, or corrode the platinum, and it would be easy to indicate a very large number of mixtures which might be so examined, and thus lead to the discovery of many definite compounds, probably thousands, which exist only whilst in solution, and are decomposed on evaporating or crystallising the solution; he has already found more than 150. The author has also employed it for ascertaining the distribution of acids and bases when together in solution, and for measuring the rate of chemical change proceeding in aqueous solutions, and generally for investigating the chemical constitution of isomeric mixtures, or those having the same ultimate chemical composition. All these results have arisen from investigating the electromotive forces of simple voltaic couples.

As an illustration of the application of the method to the examination of the internal constitution of electrolytes, including that of isomeric mixtures, two instances are given in which two mixtures, possessing the same ultimate chemical composition, exert very different amounts of voltaic energy.

In an additional note, dated December 27th, 1888, the author shows that although, according to J. Thomsen's determinations, an aqueous solution of a molecular weight proportion of $MgSO_4$, and one of K_2SO_4, neither evolve nor absorb heat on admixture with each other, distinct evidence of their chemical union whilst in solution is afforded by measurements made with the "voltaic balance;" this difference may perhaps be explained by the different degrees of sensitiveness of the two methods.

II. "Relative Amounts of Voltaic Energy of Electrolytes." By G. Gore, F.R.S. Received November 24, 1888.

(Abstract.)

In this research the author has determined by means of the "voltaic balance" the relative amounts of voltaic energy of upwards of 100 aqueous solutions of elementary substances, acids, salts, bases, organic substances, &c., exerted by them upon a simple voltaic couple at ordinary atmospheric temperature.

The method of measuring the amount of energy of a substance was as follows: Take two small glass cups containing known volumes of distilled water. Form two voltaic cells of them by means of strips or stout wires of unamalgamated zinc cut from the same piece, and two small sheets of platinum, also cut from the same piece. Connect them together in series to a sufficiently sensitive galvanometer (say, one of from 100 to 1000 ohms resistance), so that the currents from the two cells oppose each other, and produce no visible deflection of the needles. This arrangement constitutes a "voltaic balance," and is extremely sensitive to change of chemical composition of the liquid in one of the vessels. Make an aqueous solution of known strength of the substance, and add it in sufficiently small quantities at a time to the water in one of the cups until the needle of the galvanometer visibly commences to move, and note the proportion of the substance and of water then contained in that vessel.

As the amount of energy required to move the needle is the same in all cases, the different numbers thus obtained with different substances represent the relative amounts of voltaic energy of those substances. And as each substance or mixture of substances gives a different number, it is possible by this method to detect substances, to ascertain the degrees of strength or concentration of liquids, to ascertain whether a substance contains a soluble impurity, &c. The method also is in many cases an extremely sensitive one.

The names or formulæ of the substances, together with their amounts of energy, are arranged in the form of a table as a volta-tension series of electrolytes, commencing with chlorine, which gives a plus number of +1,282,000,000, and ending with caustic potash, which gives a minus one of −270,985, and a certain mixture of salts which gives −959,817.

III. "The Resistance of Electrolytes to the Passage of very rapidly alternating Currents, with some Investigations on the Times of Vibration of Electrical Systems." By J. J. THOMSON, M.A., F.R.S., Cavendish Professor of Experimental Physics, Cambridge. Received January 9, 1889.

The electromagnetic effect of the currents induced in a conducting plate by alternations in a primary electromagnetic system in its neighbourhood, is, at a point on the side of the plate opposite to the primary system, in the contrary direction to the electromagnetic effect of the primary. Such a plate, therefore, tends to shield off from a secondary system the induction due to the primary, the diminution it produces in the current induced in the secondary depending upon the conductivity and thickness of the plate and the rate of reversal of the primary current. If the rate of reversal is infinitely rapid, a thin plate of very badly conducting substance will be sufficient to screen off from the secondary circuit all the induction arising from the primary, while, if the rate is very slow, a thick plate of the best conducting metal will hardly be sufficient to do this. When the current in the primary is reversed a few hundred times per second, a metal plate of very moderate thickness will completely shield off all induction. If the thickness of the plate exceeds this limit, the currents induced in the layers next the primary will shield off all electromotive force from those layers which are more remote, so that in these layers no currents will be formed, the induced currents will thus be confined to the skin of the conductor, the thickness of the skin varying inversely as the conductivity of the plate and the rate of reversal of the current.

In Hughes' induction balance this screening effect of metal plates is made use of to compare the resistances of two metals, but with that apparatus it is hardly possible to make the alternations sufficiently rapid to produce appreciable effects with substances which conduct so badly as electrolytes; we can, however, by employing the vibrations of electrical systems such as those used by Hertz in his recent experiments on the rate of propagation of electrodynamic action get oscillations sufficiently rapid to make the shielding effect of moderately thin plates of electrolytes quite appreciable.

Before describing the experiments made on this point, we shall consider the theory of the screening effect of a slab of a conductor bounded by two parallel planes. Let us suppose that these planes are represented by the equations $x = 0$, $x = -h$; let F_1, G_1, H_1 represent the components parallel to the axes of x, y, z respectively of the vector potential on the side of the slab on which the primary system

is situated; F_2, G_2, H_2 their values in the conductor, and F_3, G_3, H_3 their values on the side of the slab remote from the primary system. Let ϕ be the electrostatic potential, and let us suppose that all the quantities vary as $e^{\iota pt}$, then

$$F = F' + \frac{\nu}{\iota p}\frac{d\phi}{dx},$$

$$G = G' + \frac{\nu}{\iota p}\frac{d\phi}{dy},$$

$$H = H' + \frac{\nu}{\iota p}\frac{d\phi}{dz},$$

$$\frac{dF'}{dx} + \frac{dG'}{dy} + \frac{dH'}{dz} = 0,$$

where ν is a constant which depends on the theory of electricity we adopt. If we assume Maxwell's theory, $\nu = 1$, and as we shall see reason later on for believing that ν has always this value, we shall henceforth in this investigation assume this value for it. F', G', H' represent transverse disturbances propagated in a dielectric with the velocity of propagation of electrodynamic action.

$$\text{Let}\quad \left.\begin{aligned} F_1 &= B_1 e^{\iota(ax+by+cz+pt)} + B_1' e^{\iota(-ax+by+cz+pt)} + \frac{1}{\iota p}\frac{d\phi}{dx} \\ G_1 &= C e^{\iota(ax+by+cz+pt)} + C' e^{\iota(-ax+by+cz+pt)} + \frac{1}{\iota p}\frac{d\phi}{dy} \\ H_1 &= D_1 e^{\iota(ax+by+cz+pt)} + D_1' e^{\iota(-ax+by+cz+pt)} + \frac{1}{\iota p}\frac{d\phi}{dz} \end{aligned}\right\} \ldots (1),$$

where the terms of the type $B_1 e^{\iota(ax+by+cz+pt)}$ represent the disturbance proceeding from the primary, and those of the type $B_1' e^{\iota(-ax+by+cz+pt)}$ the disturbance reflected from the plate.

$$\text{Let}\quad F_2 = B_2 e^{\iota(a'x+by+cz+pt)} + B_2' e^{\iota(-a'x+by+cz+pt)} + \frac{1}{\iota p}\frac{d\phi}{dx},$$

$$G_2 = C_2 e^{\iota(a'x+by+cz+pt)} + C_2' e^{\iota(-a'x+by+cz+pt)} + \frac{1}{\iota p}\frac{d\phi}{dy},$$

$$H_2 = D_2 e^{\iota(a'x+by+cz+pt)} + D_2' e^{\iota(-a'x+by+cz+pt)} + \frac{1}{\iota p}\frac{d\phi}{dz},$$

$$F_3 = B_3 e^{\iota(ax+by+cz+pt)} + \frac{1}{\iota p}\frac{d\phi}{dx},$$

$$G_3 = C_3 e^{\iota(ax+by+cz+pt)} + \frac{1}{\iota p}\frac{d\phi}{dy},$$

$$H_3 = D_3 e^{\iota(ax+by+cz+pt)} + \frac{1}{\iota p}\frac{d\phi}{dz}.$$

The boundary conditions are that F, G, H are continuous as we cross from one medium to another, that the magnetic induction at right angles to the bounding surface $dG/dz - dH/dy$ is also continuous, and that the magnetic force parallel to the surface, the components of which along the axes of y and z are respectively

$$\frac{1}{\mu}\left\{\frac{dH}{dx}-\frac{dF}{dz}\right\},$$

$$\frac{1}{\mu}\left\{\frac{dF}{dy}-\frac{dG}{dx}\right\},$$

where μ is the magnetic permeability, should also be continuous.

Let us first consider the special case where the electromotive force is everywhere parallel to the conducting plate, as this is the case which is most important for the interpretation of our experiments. In this case, B_1, B_1', B_2, B_2', and $B_3 = 0$, and we have, since G is continuous at the surface $x = 0$,

$$C_1 + C_1' = C_2 + C_2',$$

since it is continuous at $x = -h$,

$$C_3 e^{-\iota a h} = C_2 e^{-\iota a' h} + C_2' e^{\iota a' h},$$

since $dG/\mu dx$ is continuous, we have if μ' is the magnetic permeability of the plate,

$$a(C_1 - C_1') = \frac{a'}{\mu'}(C_2 - C_2'),$$

$$aC_3 e^{-\iota a h} = \frac{a'}{\mu'}(C_2 e^{-\iota a' h} - C_2' e^{\iota a' h}).$$

Solving these equations we get

$$C_1 = \frac{C_3 e^{-\iota a h}}{4aa'}\mu'\left\{\left(a+\frac{a'}{\mu'}\right)^2 e^{\iota h a'} - \left(\frac{a'}{\mu'}-a\right)^2 e^{-\iota h a'}\right\} \quad \ldots\ldots \ (2),$$

$$C_1 = \frac{C_2' e^{\iota a' h}}{a\left(\frac{a'}{\mu}-a\right)}\left\{\left(a+\frac{a'}{\mu'}\right)^2 e^{\iota h a'} - \left(\frac{a'}{\mu'}-a\right)^2 e^{-\iota h a'}\right\} \quad \ldots\ldots \ (3),$$

$$C_1 = \frac{C_2 e^{-\iota a' h}}{a\left(\frac{a'}{\mu'}+a\right)}\left\{\left(a+\frac{a'}{\mu'}\right)^2 e^{\iota h a'} - \left(\frac{a'}{\mu'}-a\right)^2 e^{-\iota h a'}\right\} \quad \ldots\ldots \ (4),$$

$$C_1 = \frac{C_1'}{e^{-\iota h a'} - e^{\iota h a'}}\left\{\frac{\left(a+\frac{a'}{\mu'}\right)}{\left(\frac{a'}{\mu'}-a\right)}e^{\iota h a'} - \frac{\left(\frac{a'}{\mu'}-a\right)}{\left(\frac{a}{\mu'}+a\right)}e^{-\iota h a'}\right\} \ldots\ldots \ (5).$$

There will be equations of exactly similar form connecting the D coefficients.

Equation (2) may be written

$$C_1 = \frac{C_3 e^{-\iota a h}\mu'}{4aa'}\left\{\left(a^2+\frac{a'^2}{\mu'^2}\right)(e^{\iota h a'}-e^{-\iota h a'})+\frac{2aa'}{\mu'}(e^{\iota h a'}+e^{-\iota h a'})\right\},$$

and if the plate is so thin that ha' is small, this may be written

$$C_1 = C_3 e^{\iota a h}\left\{\iota\frac{h}{2}\left(\mu' a+\frac{a'^2}{a\mu'}\right)+1\right\} \quad \ldots\ldots\ldots\ldots \quad (6).$$

Now the transverse disturbances satisfy in the dielectric equations of the form

$$\frac{d^2F'}{dx^2}+\frac{d^2F'}{dy^2}+\frac{d^2F'}{dz^2} = -\frac{p^2}{v^2}F',$$

where v is the velocity of propagation of the electrodynamic action; in the plate they satisfy equations of the form

$$\frac{d^2F'}{dx^2}+\frac{d^2F'}{dy^2}+\frac{d^2F'}{dz^2} = \frac{4\pi\mu'}{\sigma}\frac{dF'}{dt},$$

where σ is the specific resistance of the substance of which the plate is made.

From these equations we see that

$$a^2+b^2+c^2 = \frac{p^2}{v^2},$$

and

$$a'^2+b^2+c^2 = \frac{-4\pi\mu\iota p}{\sigma}.$$

Now if the primary system is a circular coil whose plane is parallel to the plane of the plate, b and c will be of the order π/R, where R is the radius of the coil; hence if as in our experiments $4\pi\iota p/\sigma$ is large compared with π^2/R^2, we may put

$$a'^2 = \frac{-4\pi\mu'\iota p}{\sigma}.$$

Since p^2/v^2 was small compared with b^2+c^2 for the vibrations used, we have approximately

$$a^2 = -(b^2+c^2),$$

and, therefore, a^2 is small compared with a'^2; hence from equation (6) we get

$$C_1 = -C_3 e^{-\iota a h}\left(1-\frac{2\pi h p}{a\sigma}\right)$$

or
$$C_1/C_3e^{-\iota ah} = 1-\frac{2\pi hp}{a\sigma} = \frac{2\pi\iota hp}{\sqrt{(b^2+c^2)}\sigma}+1 \;\ldots\ldots\; (7).$$

But $C_1/C_3e^{-\iota ah}$ is the proportion in which the electromotive force is reduced by the conducting plate; hence we see that if this is considerable $2\pi hp/\sqrt{(b^2+c^2)}\sigma$ must be large, and in this case the reduction is proportional to the thickness of the plate, the number of reversals in the direction of the current per second, and the specific resistance. The term b^2+c^2 will not change if the primary remains undisturbed. We see from the above investigation that if with the same rate of reversal two different plates produce the same effect upon the induced current, their thicknesses must be proportional to their specific resistances, or, in other words, the resistance of slabs of the same area to currents parallel to their bounding surfaces must be the same.

The above case is the one that is most generally useful; there is no difficulty, however, in writing down the solution of the most general case when the vector potential is not assumed to be parallel to the plate.

Using the same notation as before we have

$$C_1+C_1' = C_2+C_2',$$
$$C_3e^{-\iota ah} = C_2e^{-\iota a'h}+C_2'e^{\iota a'h},$$
$$D_1+D_1' = D_2+D_2',$$
$$D_3e^{-\iota ah} = D_2e^{-\iota a'h}+D_2'e^{\iota a'h},$$

$$c(B_1+B_1')-a(D_1-D_1') = \frac{1}{\mu'}\{c(B_2+B_2')-a'(D_2-D_2')\},$$

$$b(B_1+B_1')-a(C_1-C_1') = \frac{1}{\mu'}\{b(B_2+B_2')-a'(C_2-C_2')\},$$

$$cB_3e^{-\iota ha}-aD_3e^{-\iota ha} = \frac{1}{\mu'}\{c(B_2e^{-\iota ha'}+B_2'e^{\iota ha'}) \\ -a'(D_2e^{-\iota a'h}-D_2'e^{\iota a'h})\},$$

$$bB_3e^{-\iota ha}-aC_3e^{-\iota ha} = \frac{1}{\mu'}\{b(B_2e^{-\iota ha'}+B_2'e^{\iota ha'}) \\ -a'(C_2e^{-\iota a'h}-C_2'e^{\iota a'h})\},$$

$$aB_1+bC_1+cD_1 = 0,$$
$$-aB_1'+bC_1'+cD_1' = 0,$$
$$a'B_2+bC_2+cD_2 = 0,$$
$$-a'B_2'+bC_2'+cD_2' = 0,$$
$$aB_3+bC_3+cD_3 = 0.$$

The solutions of these equations are

$$bD_1 - cC_1 = (bD_3 - cC_3)\frac{e^{-\iota ah}\mu'}{4aa'}\left\{\left(a+\frac{a'}{\mu}\right)^2 e^{\iota h'a} - \left(\frac{a'}{\mu}-a\right)^2 e^{-\iota h'a}\right\},$$

$$bC_1 + cD_1 = (bC_3 + cD_3)\frac{e^{-\iota ah}}{2\gamma^2}\{(\gamma^2+1)^2 e^{\iota h'a} - (\gamma^2-1)^2 e^{-\iota h'a}\},$$

where
$$\gamma^2 = \frac{4\pi\mu\iota p}{\sigma p^2/v^2}.$$

From these equations we can at once find C_3 and D_3, and hence the screening effect of the plate; exactly the same conclusions hold for this as for the special case previously considered; if the screening effect of two plates is the same their thicknesses must be proportional to their specific resistance.

The rapidly alternating currents, which in the experiments were screened by the plates, were those resulting from the electrical vibrations which are set up when the electrical equilibrium of a system is disturbed. We shall now proceed to give a somewhat detailed investigation of the periods of such vibrations, as the ordinary expression for the time of vibration of a condenser, whose plates are connected by an induction coil, is not applicable to this case, and, in addition, I think the result of these investigations taken in conjunction with some experiments by Hertz, will enable us to decide the vexed question as to whether the currents flow like an incompressible fluid, and to show that Maxwell's hypothesis on this point is correct.

The case we shall investigate is that of a straight wire connecting two spherical balls. Let us take the axis of the wire as the axis of z, and let F, G, H be the components of the vector potential, ϕ the electrostatic potential.

Then
$$F = F' + \frac{\nu}{\iota p}\frac{d\phi}{dx},$$

$$G = G' + \frac{\nu}{\iota p}\frac{d\phi}{dy},$$

$$H = H' + \frac{\nu}{\iota p}\frac{d\phi}{dz},$$

where
$$\frac{dF'}{dx} + \frac{dG'}{dy} + \frac{dH'}{dz} = 0,$$

and where ν is a constant. According to Maxwell's theory $\nu = 1$, while according to v. Helmholtz's more general theory $\nu = k\omega^2$, where ω is the velocity of propagation of the electrostatic potential, and k a quantity which may be determined by the equation

$$\frac{dF}{dx}+\frac{dG}{dy}+\frac{dH}{dz}=-k\frac{d\phi}{dt}.$$

On this theory ν is also equal to $1+\frac{1}{4\pi\epsilon}$ where ϵ is a quantity such that the effect of the polarisation produced in a parallelipipedal element of dielectric by an electromotive force X may be represented by distributions of electricity of surface densities plus and minus ϵ over the faces of the parallelipipedon at right angles to X.

Let all the variable quantities be proportional to $e^{\iota(mz+pt)}$. Then in the conductor since

$$\frac{d^2H'}{dx^2}+\frac{d^2H'}{dy^2}+\frac{d^2H'}{dz^2}=\frac{4\pi\mu}{\sigma}\frac{dH'}{dt},$$

where μ is the magnetic permeability and σ the specific resistance of the conductor, we have

$$\frac{d^2H'}{dx^2}+\frac{d^2H'}{dy^2}-\left(m^2+\frac{4\pi\mu\iota p}{\sigma}\right)H'=0,$$

or, since the axis of the wire is an axis of symmetry, if r be the distance of a point in the wire from this axis

$$\frac{d^2H'}{dr^2}+\frac{1}{r}\frac{dH'}{dr}-\left(m^2+\frac{4\pi\mu\iota p}{\sigma}\right)H'=0;$$

and if
$$n^2=m^2+\frac{4\pi\mu\iota p}{\sigma},$$

the solution of the equation is

$$H'=AJ_0(\iota nr)e^{\iota(mz+pt)},$$

where $J_0(x)$ represents the Bessel's function of zero order which is finite when $x=0$.

In the dielectric surrounding the wire H′ satisfies the differential equation

$$\frac{d^2H'}{dx^2}+\frac{d^2H'}{dy^2}+\frac{d^2H'}{dz^2}=\frac{1}{v^2}\frac{d^2H'}{dt^2},$$

where v is the velocity of propagation of electrodynamic action through the dielectric. Transforming this as before, this may be written

$$\frac{d^2H'}{dr^2}+\frac{1}{r}\frac{dH'}{dr}-\kappa^2H'=0,$$

where
$$\kappa^2=m^2-$$

The solution of this is

$$H' = BI_0(\iota\kappa r)e^{\iota(mz+pt)},$$

where $I_0(x)$ is the Bessel's function of zero order which vanishes when x is infinite. We may by symmetry, since there is no current in a plane at right angles to the wire, put—

$$F' = \frac{d\chi}{dx}, \quad G' = \frac{d\chi}{dy},$$

where since $$\frac{dF'}{dx}+\frac{dG'}{dy}+\frac{dH'}{dz} = 0,$$

and F′, G′, H′ all satisfy differential equations of the same form we have in the wire

$$\chi = -\frac{\iota m}{n^2}AJ_0(\iota nr),$$

and in the dielectric

$$\chi = -\frac{\iota m}{\kappa^2}BI_0(\iota\kappa r).$$

Again if ω and ω' are the velocities of propagation of the electrostatic potential in the wire and dielectric respectively, we have in the wire

$$\phi = CJ_0(\iota qr),$$

where $$q^2 = m^2-\frac{p^2}{\omega^2};$$

and in the dielectric $$\phi = DI_0(\iota q'r),$$

where $$q'^2 = m^2-\frac{p^2}{\omega'^2}.$$

Since ϕ is continuous as we cross from the wire to the dielectric, we have if a be the radius of the wire

$$CJ_0(\iota qa) = DI_0(\iota q'a) \;\dots\dots\dots\dots\dots\; (8),$$

and since H is continuous, we have

$$AJ_0(\iota na)-BI_0(\iota\kappa a) = \frac{(\nu'-\nu)}{\iota p}\,\iota mCJ_0(\iota qa)\dots\dots\;(9),$$

where ν and ν' are the values of ν in the wire and dielectric respectively. Since F and G are continuous, we have

$$-\frac{\iota m}{n^2}\,\iota nAJ_0'(\iota na)+\frac{\nu}{\iota p}\,\iota qCJ_0'(\iota qa) = -\frac{\iota m}{\kappa^2}\,\iota\kappa BI_0'(\iota\kappa a)+\frac{\nu'}{\iota p}\,D\iota q'DI_0'(\iota q'a),$$

or

$$m\left\{\frac{A}{n}J_0'(\iota na)-\frac{B}{\kappa}I_0'(\iota\kappa a)\right\}=\frac{1}{p}\{\nu'q'DI_0'(\iota q'a)-\nu qCJ_0'(\iota qa)\}\,..\,(10).$$

Since the magnetic force parallel to the surface of the wire is continuous,

$$\frac{1}{\mu}\left\{\frac{d}{dz}\frac{d\chi}{dr}-\frac{dH}{dr}\right\}$$

is continuous, and therefore

$$\frac{\iota m}{\mu n^2}\cdot\iota m\,.\,\iota nAJ_0'(\iota na)-\frac{1}{\mu}\iota nAJ_0'(\iota na)=\frac{\iota m}{\kappa^2}\iota m\,\iota\kappa\,BI_0'(\iota\kappa a)-\iota\kappa\,BI_0'(\iota\kappa a),$$

or $$AJ_0'(\iota na)\frac{(m^2-n^2)}{\mu n}=BI_0'(\iota\kappa a)\frac{m^2-\kappa^2}{\kappa}\,\ldots\ldots\ldots\,(11).$$

From equations (9) and (11) we get

$$A\left(J_0(\iota na)-J_0'(\iota na)\frac{I_0(\iota\kappa a)}{I_0'(\iota\kappa a)}\frac{m^2-n^2}{\mu(m^2-\kappa^2)}\frac{\kappa}{n}\right)=\frac{\nu'-\nu}{p}mCJ_0(\iota qa)\,..\,(12),$$

and from (10) and (11) we get

$$AJ_0'(\iota na)\frac{m}{n}\left\{1-\frac{(m^2-n^2)}{\mu(m^2-\kappa^2)}\right\}=\frac{C}{p}\left(\nu'q'\frac{I_0'(\iota q'a)}{I_0(\iota q'a)}J_0(\iota qa)-\nu qJ_0'(\iota qa)\right)$$
(13).

Hence, eliminating A and C from these equations, we get

$$\frac{n}{m}\frac{J_0(\iota na)}{J_0'(\iota na)}-\frac{I_0(\iota\kappa a)}{I_0'(\iota\kappa a)}\frac{(m^2-n^2)}{\mu(m^2-\kappa^2)}\frac{\kappa}{m}=\frac{(\nu'-\nu)m\left\{1-\dfrac{(m^2-n^2)}{\mu(m^2-\kappa^2)}\right\}}{\nu'q'\dfrac{I_0'(\iota q'a)}{I_0(\iota q'a)}-\nu q\dfrac{J_0'(\iota qa)}{J_0(\iota qa)}}\,..\,(14).$$

In the cases dealt with in these experiments the rate of vibration was so rapid that na was very large. In this case $J_0'(\iota na)=\iota J_0(\iota na)$. If Maxwell's theory is true, the right hand side vanishes, since $\nu'=\nu$, and we have

$$\frac{\iota n}{m}=\frac{I_0(\iota\kappa a)}{I_0'(\iota\kappa a)}\frac{4\pi\iota pv^2}{\sigma p^2}\frac{\kappa}{m},$$

or $$\kappa\frac{I_0(\iota\kappa a)}{I_0'(\iota\kappa a)}=\frac{\sigma pn}{v^2 4\pi}\,\ldots\ldots\ldots\ldots\ldots\ldots\,(15).$$

The right hand side of this equation is very small, so κ must be very small. In this case

$$I_0(\iota\kappa a)=\log\gamma\iota\kappa a \text{ approximately},$$

where $$\log\gamma = 0{\cdot}577 - \log 2,$$

so that equation (14) becomes

$$\iota\kappa^2 a \log\gamma\iota\kappa a = \frac{\sigma p n}{4\pi v^2},$$

the solution of which (see 'London Math. Soc. Proc.,' vol. 17, p. 316) is

$$\kappa^2 = \frac{p\sigma}{4\pi v^2 a}\sqrt{\frac{2\pi\mu p}{\sigma}}\frac{(1-\iota)}{\log\frac{p\sigma a\gamma^2}{2\pi v^2}\left\{\frac{2\pi\mu p}{\sigma}\right\}^{\frac{1}{2}}},$$

and therefore

$$m^2 = \frac{p^2}{v^2} + \frac{p\sigma}{4\pi v^2 a}\sqrt{\frac{2\pi\mu p}{\sigma}}\frac{(1-\iota)}{\log\left[\frac{p\sigma a\gamma^2}{2\pi v^2}\left\{\frac{2\pi\mu p}{\sigma}\right\}^{\frac{1}{2}}\right]}$$

Thus in this case, since the second term on the right-hand side is small compared with the first, the disturbance is propagated along the wire with the same velocity as that of electrodynamic action through the dielectric. The amplitude of the vibration will sink to $1/e$ of its original value after traversing a distance

$$8va\left\{\frac{\pi}{2\mu\sigma p}\right\}^{\frac{1}{2}}\log\left[\frac{ap^{\frac{1}{2}}\gamma^2}{v^2}\left(\frac{\sigma}{4\pi\mu}\right)^{\frac{1}{2}}\right].$$

If, however, $\nu'-\nu$ does not vanish, and if we suppose qa small, which will be the case unless the velocity of propagation of the electrostatic potential is exceedingly small compared with that of electrodynamic action, since in this case

$$\nu' q' \frac{\mathrm{I}_0'(\iota q'a)}{\mathrm{I}_0(\iota qa)} - \nu q \frac{\mathrm{J}_0'(\iota q'a)}{\mathrm{J}_0(\iota qa)} = \frac{\nu'}{\iota a \log(\gamma\iota qa)},$$

and since $\dfrac{m^2-n^2}{\mu(m^2-\kappa^2)}$ is very large compared with unity, equation (14) becomes, remembering that na is large,

$$\frac{\iota n}{m} - \frac{\mathrm{I}_0(\iota\kappa a)}{\mathrm{I}_0'(\iota\kappa a)}\cdot\frac{4\pi\iota v^2}{\sigma p}\frac{\kappa}{m} = \frac{4\pi\iota v^2}{\sigma p}\frac{m(\nu'-\nu)}{\nu'}\iota a\log\gamma\iota qa \quad \ldots.(16);$$

and unless $(\nu'-\nu)/\nu$ be very small, the right hand side in this equation is very large compared with the first term on the left, and the equation becomes

$$\frac{\mathrm{I}_0(\iota\kappa a)}{\mathrm{I}'_0(\iota\kappa a)}\frac{\kappa}{m} = \frac{-m(\nu'-\nu)}{\nu'}\iota a\log\gamma\iota qa.$$

The right hand side is small so that κa will be small, and the equation to determine it

$$\kappa^2 a \log \gamma\iota\kappa a = -m^2\frac{(\nu'-\nu)}{\nu'}\iota a \log \gamma\iota q a,$$

or
$$\kappa^2 \log \gamma\iota\kappa a = -m^2\frac{(\nu'-\nu)}{\nu'} \log \gamma\iota q a.$$

The solution of this equation is approximately

$$\kappa^2 = \frac{-m^2\frac{(\nu'-\nu)}{\nu'}\log \gamma\iota q a}{\log\left(2\gamma^2 m^2 a^2\frac{(\nu'-\nu)}{\nu'}\log \gamma\iota q a\right)},$$

or say
$$\kappa^2 = -\beta m^2;$$

but
$$\kappa^2 = m^2 - \frac{p^2}{v^2},$$

therefore
$$m^2(1+\beta) = \frac{p^2}{v^2},$$

and the velocity of propagation of the disturbance through the wire is p/m or $v\sqrt{(1+\beta)}$. Since the imaginary part of m does not involve σ, and a only occurs under the logarithm, the rate at which the vibrations die away will in this case be practically independent of the resistance and size of the wire. Thus, unless Maxwell's theory is true, the rate of propagation of a very rapidly alternating disturbance through a conductor is not the same as that of the electrodynamic action through the surrounding dielectric; if β is positive it goes faster through the wire than through the dielectric, while if β is negative it goes more slowly. The rate of propagation through the wire is almost though not quite independent of the size and conductivity of the wire and of the rapidity of the vibrations. Thus, if it could be proved that the velocity of a disturbance through a conducting wire differed appreciably from the velocity of electrodynamic action, and that the rate at which the vibrations die away did not depend upon the resistance, it would be sufficient to show that Maxwell's assumption is untenable. Hertz's experiments would seem to show that the rate of propagation through a metallic wire is less than that of electrodynamic action through the dielectric; but I believe he has lately found that the former rate increases rapidly with rapidity of the vibrations, which is inconsistent with the above result, if ν' and ν are independent of p. No experiments seem to have been made on the rate at which the vibrations die away, though this would be one of the best ways of distinguishing between the theories.

If we suppose that the rate of propagation of the electrostatic potential is exceedingly small, q and q' will be very large, so that unless $\nu' q' = \nu q$, the denominator of the right hand of (15) will be exceedingly large, so that the case is the same as when $\nu' = \nu$, and therefore the rate of propagation of a disturbance through a wire the same as that of electrodynamic action through air.

We shall now investigate the time of vibration of a system consisting of a straight wire connecting two spherical balls. Let us take the middle of the wire as the origin, and suppose that the flow of electricity is symmetrical about this point; at points equidistant from the origin the electrostatic potential will be equal and opposite.

Using the same notation as before, let

$$\phi = \mathrm{C}(e^{\iota mz} - e^{-\iota mz})e^{\iota pt}\mathrm{J}_0(\iota qr) \text{ in the wire,}$$

$$= \mathrm{D}(e^{\iota mz} - e^{-\iota mz})e^{\iota pt}\mathrm{I}_0(\iota q'r) \text{ in the dielectric,}$$

$$\mathrm{H} = \mathrm{A}(e^{\iota mz} + e^{-\iota mz})e^{\iota pt}\mathrm{J}_0(\iota nr) + \frac{\nu}{\iota p}\frac{d\phi}{dz} \text{ in the wire,}$$

$$= \mathrm{B}(e^{\iota mz} + e^{-\iota mz})e^{\iota pt}\mathrm{I}_0(\iota\kappa a) + \frac{\nu'}{\iota p}\frac{d\phi}{dz} \text{ in the dielectric.}$$

If w is the intensity of the current parallel to the axis of z,

$$\sigma w = -\frac{d\mathrm{H}}{dt} + \frac{d\phi}{dz}$$

$$= -\mathrm{A}\iota p(e^{\iota mz} + e^{-\iota mz})e^{\iota pt}\mathrm{J}_0(\iota nr) - (\nu - 1)\mathrm{C}\iota m(e^{\iota mz} + e^{-\iota mz})$$
$$e^{\iota pt}\mathrm{J}_0(\iota qr).$$

The quantity of electricity Q which has passed across any section at right angles to the axis is given by

$$\frac{d\mathrm{Q}}{dt} = \int_0^a 2w\pi r dr,$$

since
$$\frac{d^2\mathrm{J}_0(\iota nr)}{dr^2} + \frac{1}{r}\frac{d\mathrm{J}_0(\iota nr)}{dr} = n^2\mathrm{J}_0(\iota nr),$$

$$\int_0^a r\mathrm{J}_0(\iota nr)dr = \frac{1}{n^2}a\iota n\mathrm{J}_0'(\iota na) = \frac{\iota a}{n}\mathrm{J}_0'(\iota na),$$

we see that

$$\frac{\sigma d\mathrm{Q}}{dt} = -2\pi\mathrm{A}\iota p(e^{\iota mz} + e^{-\iota mz})e^{\iota pt}\,\frac{\iota a}{n}\mathrm{J}_0(\iota na)$$
$$-(\nu - 1)2\pi\mathrm{C}\iota m(e^{\iota mz} + e^{-\iota mz})e^{\iota pt}\frac{\iota a}{q}\mathrm{J}_0'(\iota qa).$$

If the ends of the wire are given by $z = \pm l$, the rate at which electricity flows across the end is given by

$$\frac{\sigma dQ}{dt} = 2\mathrm{A}\alpha . \frac{p}{n} \cos ml\, e^{\iota pt} \mathrm{J}_0'(\iota n a) - (\nu - 1)\, 2\mathrm{C} . \frac{ma}{q} \cos ml\, e^{\iota pt} \mathrm{J}_0'(\iota q a),$$

or by equation (13)

$$\sigma \frac{dQ}{dt} = \frac{4\pi \mathrm{C} a e^{\iota pt} \cos ml}{m \left\{ 1 - \dfrac{(m^2 - n^2)}{\mu(m^2 - \kappa^2)} \right\}} \cdot \left\{ \nu' q' \frac{\mathrm{I}_0'(\iota q' a)}{\mathrm{I}_0(\iota q' a)} \mathrm{J}_0(\iota q a) - \nu q \mathrm{J}_0'(\iota q a) \right\}$$

$$- (\nu - 1) 4\pi \mathrm{C} m \frac{a}{q} e^{\iota pt} \cos ml\, \mathrm{J}_0'(\iota q a);$$

if, however, α is the capacity (in electromagnetic measure) of the condenser at the end $z = l$

$$\mathrm{Q} = \alpha \mathrm{C} (e^{\iota ml} - e^{-\iota ml}) e^{\iota pt} \mathrm{J}_0(\iota q a);$$

so that

$$\sigma \frac{dQ}{dt} = \sigma \alpha \iota p \mathrm{C} (e^{\iota ml} - e^{-\iota ml}) e^{\iota pt} \mathrm{J}_0(\iota q a)$$

$$= -2\alpha\sigma p \sin ml\, \mathrm{C} e^{\iota pt} \mathrm{J}_0(\iota q a).$$

Equating these expressions for $\sigma \dfrac{dQ}{dt}$ we get

$$-2\alpha\sigma p \sin ml\, \mathrm{C} e^{\iota pt} \mathrm{J}_0(\iota q a) = \frac{4\pi \mathrm{C} a e^{\iota pt} \cos ml}{m \left\{ 1 - \dfrac{(m^2 - n^2)}{\mu(m^2 - \kappa^2)} \right\}} \left\{ \nu' q' \frac{\mathrm{I}_0'(\iota q' a)}{\mathrm{I}_0(\iota q' a)} \mathrm{J}_0(\iota q a) \right.$$

$$\left. - \nu q\, \mathrm{J}_0'(\iota q a) \right\} - 4\pi(\nu - 1) \frac{\mathrm{C} m a}{q} e^{\iota pt} \cos ml\, \mathrm{J}_0'(\iota q a)$$

$$-p\sigma m \alpha \tan ml \left\{ 1 - \frac{(m^2 - n^2)}{\mu(m^2 - \kappa^2)} \right\} = \frac{2\pi \nu' q' a \mathrm{I}_0'(\iota q' a)}{\mathrm{I}_0(\iota q' a)} - \frac{2\pi \nu q a \mathrm{J}_0'(\iota q a)}{\mathrm{J}_0(\iota q a)}$$

$$-2\pi(\nu - 1) \frac{m^2 a}{q} \frac{\mathrm{J}_0'(\iota q a)}{\mathrm{J}_0(\iota q a)} \left\{ 1 - \frac{(m^2 - n^2)}{\mu(m^2 - \kappa^2)} \right\} \ldots . (17).$$

Since

$$\frac{m^2 - n^2}{\mu(m^2 - \kappa^2)} = -\frac{4\pi \iota p}{\sigma \dfrac{p^2}{v^2}},$$

it is very large compared with unity, and if qa and $q'a$ are small,

$$\nu' q' a \frac{\mathrm{I}_0'(\iota q' a)}{\mathrm{I}_0(\iota q' a)} - \nu q a \frac{\mathrm{J}_0'(\iota q a)}{\mathrm{J}_0(\iota q a)} = \frac{\iota \nu'}{\log(\gamma \iota q' a)},$$

and

$$\frac{a}{q} \frac{\mathrm{J}_0'(\iota q a)}{\mathrm{J}_0(\iota q a)} = -\tfrac{1}{2} a^2 \iota.$$

This equation (17) reduces to

$$-4\pi\iota v^2\alpha m \tan ml = \frac{2\pi\iota v'}{\log(\gamma\iota q'a)} - \frac{(\nu-1)m^2a^2}{\sigma p}v^2 4\pi^2 \;\ldots\ldots\; (18),$$

if $\nu = 1$, that is, if Maxwell's theory is true,

$$2v^2\alpha m \tan ml = -\frac{1}{\log(\gamma\iota q'a)}.$$

Now $v^2\alpha$ is the electrostatic measure of the capacity, so that if we denote this by $\{\alpha\}$,

$$ml \tan ml = \frac{l}{2\{\alpha\}\log(1/\gamma'q'a)}.$$

The form of the solution will depend upon the magnitude of $l/2\{\alpha\}\log(1/\gamma q'a)$. If this is small then ml will be small, and we have

$$m^2l^2 = \frac{l}{2\{\alpha\}\log(1/\gamma\iota'q'a)},$$

or

$$m = \frac{1}{\sqrt{2l\{\alpha\}\log(1/\gamma'ma)}},$$

since, if Maxwell's theory be true, $q' = m$.

This result, however, is only true when l is not large compared with α, in this case ml tan ml will be large, and m therefore will be approximately $\frac{1}{2}\pi$, $\frac{3}{2}\pi$, and so on. Thus in this case the ends of the wire are nodes of the electrical vibrations, and the gravest mode of vibration is that in which the wave-length is twice the length of the wire; here the wave-length, and therefore the rapidity of vibration, will be independent of the capacities of the condensers at the ends.

If $\nu-1$ is finite, since the second term on the right hand side of equation (17) will in this case be large compared with the first, since pa^2/σ is large, the equation reduces to—

$$\iota v^2\alpha m \tan ml = \frac{(\nu-1)m^2a^2v^2\pi}{\sigma p};$$

or since $p = (1+\beta)vm$,

$$\iota\{\alpha\}m \tan ml = \frac{(\nu-1)pa^2\pi}{\sigma(1+\beta)^2}.$$

Now in the cases we are considering $p\pi a^2/\sigma$ is very large, amounting to 10^4 or 10^5 in the C.G.S. system of units, so that unless $\{\alpha\}$ is comparable with 1/10 of a microfarad ml will equal $\pi/2$, the ends of the wire will again be nodes, and the wave-length of the gravest vibration will be twice the length of the wire. Thus in this case, except

the capacity of the condenser were exceedingly large, much greater than that requisite for the same purpose in the preceding case, the time of vibration would be independent of the capacities of the ends; and conversely, if we could prove that the time of vibration depends upon the capacity, we should prove that $\nu = 1$. Now Hertz in his experiments seems to have been able to bring two circuits into resonance by altering the capacity of the ends, though these capacities were exceedingly small compared with 1/10 of a microfarad. This, therefore, is exceedingly strong testimony in favour of the truth of Maxwell's theory, at any rate for conductors.

[*Note added February* 15, 1889.—We can find the ratio of ν_1 to ν_2, the values of ν for a dielectric and conductor respectively, by considering the reflection of an electromagnetic disturbance at a metallic surface. Using the notation of the beginning of the paper, let the incident waves of the vector potential be expressed by

$$F' = Ae^{\iota(ax+by+cz)},$$

$$G' = Be^{\iota(ax+by+cz)},$$

$$H' = Ce^{\iota(ax+by+cz)};$$

the reflected waves by

$$F_1' = A'e^{\iota(-ax+by+cz)},$$

$$G_1' = B'e^{\iota(-ax+by+cz)},$$

$$H_1' = C'e^{\iota(-ax+by+cz)}.$$

Then assuming that $4\pi\mu\iota p/\sigma$, b^2+c^2 are large compared with p^2/v^2, we find—

$$\frac{A'}{A} = \frac{\nu_2}{\nu_1},$$

$$B'+B = \frac{\nu_2-\nu_1}{\nu_1} A \frac{b}{\sqrt{(b^2+c^2)}},$$

$$C'+C = \frac{\nu_2-\nu_1}{\nu_1} A \frac{b}{\sqrt{(b^2+c^2)}}.$$

Thus the electromotive force parallel to the surface of the reflector does not vanish at the surface unless $\nu_2 = \nu_1$. Hertz ('Wied. Ann.,' 34, 615) found that when the plane of the secondary circuit was parallel to the reflecting surface, the sparks vanished at the reflecting surface, thus showing that $\nu_2-\nu_1$ is at any rate small. The method founded on the law of decay of the vibrations is more delicate, as it shows whether or not $(\nu_2-\nu_1)na$ is small and na is a large quantity.]

In the above work we have assumed that qa is small, but if qa be

large, as would be case if the rate of propagation of the electrostatic potential were exceedingly small compared with that of electrodynamic action, the first term on the right hand side of equation (14) would be very large, so that in this case again tan ml would be large and $ml = \frac{1}{2}\pi$ approximately, and the same arguments would apply as in the case when $\nu - 1$ was finite.

If $\nu = 1$ for all substances, then since the electromotive force parallel to the axis of x is $-dF/dt - d\phi/dx$, and since $F = F' + d\phi/dx \cdot \iota p$, the x component of the electromotive force is $-dF'/dt$. Similarly, the y and z components are $-dG'/dt$, $-dH'/dt$. Thus the electromotive force is propagated with the velocity of the transverse vibrations (see "Report on Electrical Theories," 'Brit. Assoc. Report,' Aberdeen, 1885, p. 138), and since F′, G′, H′ satisfy the solenoidal condition, there is no condensation.

The rate of propagation of a disturbance through a conductor is only equal to that of the electrodynamic action through a dielectric when $\sigma/\pi p a^2 \log p\sigma\gamma^2/\pi\mu v^2$ is small, and though this will be so for the rapid vibrations we are dealing with when the conductor is metallic, it would not be so if the conductor were a dilute electrolyte or a rarefied gas. In this case the rate of propagation of the disturbance through the conductor would not be the same as that through the dielectric. In this case the action propagated along the conductor, and that propagated through the dielectric, would when they met interfere and set up standing vibrations, so that along the conductor there would be a series of stationary nodes at which the current vanished, in other words, the current along the conductor would be striated. In the discharge of electricity through rarefied gases we have the current passing through a conductor of high resistance, and it seems possible that the striations which are observed in the case may be due to the interference of the disturbance propagated through the conducting gas and that passing through the dielectric. The widening of the striæ on rarefaction, and on increasing the diameter of the discharge tube, are consistent with this view.

The resistances of the electrolytes to the very rapidly alternating currents were compared in the following way :—

A, B, C are three coils, two of which (B and C) are approximately of the same dimensions, and are nearly but not quite closed. Spherical balls are fastened to the ends of these coils. The two balls of

the coil C are supported in an ebonite frame provided with an ebonite screw, by means of which the two balls can be brought very near together and kept so as long as is necessary.

The coils B and C are placed on shelves of glass coated with shellac. The shelves are supported on a framework with supports at different levels, as in an ordinary book-case, so as to enable the distance between the primary and secondary coils B and C to be altered if necessary. The coil A is connected to an induction coil which, when in good order, will give sparks 5 or 6 inches long. The coil is worked by a slow mercury break, the speed of which can be regulated by altering the inclination of the arms of a fan whose motion resists that of the break: in the actual experiments the circuit was broken every few seconds. When the coil works sparks pass between the points *e* and *f*, electrical vibrations are started in the coil B; in other words, there are alternating currents in B whose period is that of its electrical vibration, and given by equation (18). The currents in B will induce currents in C, and these latter will be rendered evident by the production of a minute spark between the two balls at its extremities. These sparks, though small, are so bright that they can be readily observed without darkening the room.

The production of sparks in the secondary circuit is much affected by what are, apparently, slight alterations in the conditions of the primary. Thus, for example, it is very much facilitated by placing the balls of a pair of discharging tongs between *e* and *f*, and allowing the spark to jump from *e* to the discharging tongs, and then from the discharging tongs to *f*. This change did not seem to be due to the resonance between the coils B and C being improved by the presence of the tongs, for unless they were placed in the way of the spark they produced no effect; again, it was not altogether due to an increase in the quality of electricity which passed from A to B at each discharge, as this was measured by placing a specially insulated galvanometer in the circuit, and it was sometimes found that the quantity of electricity which passed when the tongs were not interposed and when no spark was produced in the secondary circuit, was greater than the amount which passed when the tongs were interposed and when sparks were produced. The character of the spark which passes between A and B has also great influence—the best sparks are those which are perfectly straight, and accompanied by a sharp snap; zigzag sparks in the primary very rarely produce any sparks in the secondary.

A conducting plate placed between B and C ought, as we have seen, to diminish the induction between them, and therefore the electromotive force in the circuit C, and since the diminution in the induction increases with the rapidity with which the current in the primary is reversed, it ought in this case to be very marked. This was found to be

the case; thin sheet metal and tin-foil when placed between the coils were found to completely stop the sparks in C. I then coated a plate of glass, which of itself had no effect upon the sparks, with a film of Dutch metal about $\frac{1}{1100}$ of a centimetre in thickness, and found that it completely stopped the sparks, and I have not been able to get a film of metal thin enough to allow sufficient induction to pass through to produce sparks in the secondary.

This is in accordance with the results of our investigation on the screening effect of conducting plates, for we saw by equation (7) that when a screen of thickness h was interposed the electromotive force is only

$$\frac{\sqrt{(b^2+c^2)}\sigma}{2\pi hp},$$

when σ is the conductivity of the metal.

Since the electromotive force in the plane of the screen, which is taken as the plane of yz, is of the form

$$\Sigma \cos by \cos cz,$$

$\sqrt{(b^2+c^2)}$ will be of the order $2\pi/R$ where R is the radius of the primary coil; several coils were used whose radii varied from 13 to 23 c., so that $\sqrt{(b^2+c^2)}$ will be of the order 1/2. The length of the coils varied from 81 to 140 c., and the balls at the extremities from 1 to 2 c. in diameter, so that the length divided by the capacity is large, and, therefore, by equation (18) the wave-length will be twice the length of the coil, or for the largest coil about 3 metres; thus p will be about $2\pi \times 10^8$, and if we suppose the film is $\frac{1}{1000}$ of a millimetre thick, h will be 5×10^{-4}, we may take σ to be 10^4. A film of this kind will, by the above formula, diminish the induction about 800 times, and we should, therefore, not expect the electromotive force acting on the secondary to be sufficient to produce sparks.

A thick plate of ebonite was next placed between the coils but did not produce any appreciable diminution in the sparks in the secondary; thus ebonite, though opaque to vibrations as rapid as those of light, still allows vibrations of which 10^8 take place in a second to pass through without interruption.

The effect of interposing a film of electrolyte was next tried. A large square glass trough was placed between the coils B and C and carefully levelled, the electrolyte was then poured in; when only a very small quantity of electrolyte was in the trough the sparks still passed, but they got feebler and feebler as the quantity of electrolyte in the trough increased, until finally, when the electrolyte was dilute sulphuric acid, they ceased altogether when the depth ot the electrolyte in the trough amounted to 3 or 4 millimetres. The criterion adopted for the disappearance of the sparks was to allow 60 sparks to pass into the

primary, stopping and starting the coils several times; if, during this time, no sparks passed in the secondary, the sparks were considered stopped. A variation of 5 per cent. in the quantity of electrolyte present would cause the system to pass this point one way or another in a marked way.

The balls at the extremity of the secondary were adjusted so that sparks passed freely before the electrolyte was put in, after each experiment the electrolyte was removed, and care was taken to ascertain that sparks still passed as freely as before so as to guard against any accidental disarrangement of the secondary during the experiment.

Three sets of coils were used which we shall describe by I, II, III. Set I consisted of two circular brass coils, 140·8 c. in circumference. The diameter of the brass rod of which they were made was about 0·6 c.; the balls at the extremities were 2 c. in diameter. The time of vibration of this coil, calculated by equation (18), is about 10^{-8} seconds.

Set II consisted of two circular copper coils 81·2 c. in circumference, the rod of which they were made being about 0·5 c. in diameter; the balls at the extremities were 1 c. in diameter. The time of vibration is about 5×10^{-9} seconds. With these small coils the balls of the secondary had to be exceedingly close together in order to get sparks, but when the micrometer screw was properly adjusted the sparks were very bright and the indications quite definite. The coils were about 9 c. apart.

Set III consisted of two rectangular coils made of sheet lead, one side was 30 c., the other 40, the breadth of the sides was 5 c., and the diameter of the balls at the extremity 2. The distance between the coils was 15 c. The time of vibration about 10^{-8} seconds.

The electrolytes used were solutions of—

Sulphuric acid,	specific gravity of solution		....	1·175
Ammonium chloride	,,	,,	....	1·072
Sodium ,,	,,	,,	....	1·185
Potassium ,,	,,	,,	....	1·155
Ammonium nitrate	,,	,,	....	1·175
Potassium carbonate	,,	,,	....	1·280

In the following table the relative thickness of the films of these substances required to stop the spark is given, each number being the mean of several observations. The thickness of the H_2SO_4 film was taken as unity. An observation with sulphuric acid was made before and after the observation with any other electrolyte.

	H_2SO_4.	NH_4Cl.	NaCl.	KCl.	NH_4NO_3.	K_2CO_3.
Coil I	1	1·45	2·4	2·5	1·6	3·1
Coil II	1	1·5	2·5	3·3	1·9	3·1
Coil III	1	1·63	2·75	3·2	2·0	3·3
Mean............	1	1·53	2·55	3·0	1·8	3·2
Relative resistance with very slow reversal.	1	1·65	3·0	3·4	1·8	2·8

The thicknesses of the films are by equation (7) proportional to the specific resistances, so that the numbers in the fourth line give the relative resistance of the electrolytes to currents whose directions are reversed from 10^8 to 2×10^8 times per second. In order to see whether these resistances are the same as those with an almost infinitely slower reversal, I determined the resistance of the electrolytes by using a commutator which reversed the current through the electrolyte about 120 times a second, and kept the direction of the current through the galvanometer constant. The electrodes were platinised, and no polarisation could be detected. The numbers are given in the last line of the above table, and agree sufficiently well to enable us to say that the relative resistance of electrolytes is the same when the current is reversed a hundred million times a second as for steady currents.

It was not possible to compare in this way the resistances of electrolytes and metals, as the thinnest metallic film which could be obtained was evidently much thicker than was necessary to completely stop all induction. I succeeded, however, in comparing by this method the resistances of graphite and sulphuric acid. The graphite film was prepared by placing a sheet of glass at the bottom of a trough filled with water, holding a large quantity of finely powdered graphite in suspension; after the graphite had deposited itself uniformly on the glass plate, the water was syphoned off, and the graphite film allowed to dry gradually. When quite dry it was hard and compact, and could be rubbed down by emery to any required thickness. By diminishing the thickness of the film and adjusting the distance between the coils, a film of graphite was obtained which just stopped the sparks; a film of H_2SO_4 was then substituted for the graphite, and its thickness adjusted until it, too, just stopped the sparks. In this case, by formula (7) the resistance of equal and similar areas of the two films to currents parallel to their surface must be the same, the currents being reversed 10^8 times per second. I determined the resistances to steady currents parallel to the surface, and found that

the resistance of the graphite film was 6·7 ohms, and that of the sulphuric acid 7·2 ohms; thus we may say that the ratio of the specific resistances of graphite and sulphuric acid is the same when the currents are steady as when they are reversed 10^8 times per second. Since the ratio of the resistances of such dissimilar things as graphite and electrolytes remains the same, we may conclude that the resistances themselves remain unaltered. The method described above for comparing the resistances of electrolytes is one that can be very easily and quickly applied, and only requires the simplest apparatus: an induction coil, or, if that is not available, an electrical machine, being all that is required. The method has the advantage of avoiding the use of electrodes, as all the circuits in the electrolyte are closed.

Since electrolytes are transparent, they must, if the electromagnetic theory of light is true, act as insulators when the currents are reversed as often as light vibrations, or about 10^{15} times per second. We have seen, however, that they conduct as well when the currents are reversed 10^8 times a second as when they are steady; thus the molecular processes which cause electrolytic conduction must occupy a time between 10^{-8} and 10^{-15} seconds.

Another point which can be settled by this method is, whether a vacuum is a conductor or an insulator. According to one view the great resistance which a highly exhausted vessel offers to the passage of electricity is due to the difficulty of getting the current from the electrode into the rarefied gas: when once the current has got there, there is, according to this theory, no further resistance to its passage: if this theory is correct, a highly exhausted receiver placed between the primary and secondary circuits ought to stop the sparks in the latter, as since all the circuits are closed there ought to be no obstacle to the passage of the induced currents. In order to test this I took a box, 50 c. by 50 c. by 4 c., the top and bottom of which were sheets of plate-glass fitting into wooden sides; the sheets of glass were also supported by five ebonite pillars placed at equal intervals over their surface. The box was repeatedly dipped into a bath filled with melted paraffin until it was surrounded by a coating of paraffin about 2 c. thick. The paraffin was then smoothed over with a hot soldering iron, and then covered with a layer of shellac varnish. The box was then exhausted by a mercury-pump, and it was found that the pressure could be reduced to about 1 mm. of mercury, but no further. When this vacuum was placed between the primary and secondary coils it did not produce the slightest effect upon the sparks, so that its conductivity must be very small indeed compared with that of the electrolytes used in the preceding experiments. I am having an earthenware vessel made with which I hope to repeat the experiment at much higher exhaustions.

I also tried whether the conductivity of the electrolyte was altered

by sending a current through it; for this purpose a layer of sulphuric acid was placed between the primary and secondary coils of such thickness that it almost but not quite stopped the sparks in the latter; a current of about 2 ampères, which was reversed about 500 times a second, was then sent through the sulphuric acid, but the passage of the current did not seem to produce any effect whatever upon the sparks in the secondary. I conclude, therefore that the resistance of an electrolyte is not affected by the passage of a current.

I wish to express my thanks to my assistant, Mr. E. Everett, for the zeal and skill he has displayed in these experiments.

[*Note added February* 15, 1889.—I have recently tried the effect of a very high vacuum in stopping the sparks. The primary circuit consisted of two straight wires with spheres fastened to one end of each; these wires were connected with the poles of an induction coil, and the sparks passed between the spheres. The secondary consisted of two similar wires, with smaller balls at the ends, the distance between the balls being very small. The length of the wires of the secondary was altered until it was in resonance with the primary. The secondary was placed in a hollow cylinder formed of two coaxial glass tubes, sealed on to a mercury pump, by means of which a very high vacuum was obtained in the space between them, which surrounded the secondary. This vacuum, however, did not produce the slightest effect on the sparks.]

Presents, January 17, 1889.

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The Sun's Surface as observed by James Nasmyth, June 5th, 1864. Photographed from the original drawing. Mr. Nasmyth.

January 24, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Influence of Carbonic Anhydride and other Gases on the Development of Micro-organisms." By PERCY F. FRANKLAND, Ph.D., B.Sc. (Lond.), F.I.C., Assoc. Roy. Sch. of Mines, Professor of Chemistry in University College, Dundee. Communicated by Professor T. E. THORPE, Ph.D., F.R.S. Received December 18, 1888.

In consequence of a paper which has appeared in the last number of the 'Zeitschrift für Hygiene,' by Dr. Carl Fränkel, entitled "Ueber die Einwirkung der Kohlensäure auf die Lebensthätigkeit der Mikroorganismen," I have been led to publish the results of some preliminary experiments on the same subject which I made in the spring of 1886, but which, owing to my attention being at that time devoted to investigations in other directions, I was obliged to put on one side. Although the methods which I adopted in my experiments are essentially different from those which Fränkel has employed, yet the results, so far as they can be compared with his, are on the whole concordant.

In my experiments I used the ordinary methods of plate-cultivation (Esmarch's important modification having not yet been published), the plate-cultivations of the various micro-organisms being then submitted to an atmosphere of different gases in the following manner:—A suitable attenuation of a particular micro-organism was employed, and gelatine plates were poured in the usual way; the different plates, resting one above the other on small glass stages, were placed in a flat porcelain dish and covered over with a glass bell-jar. Mercury was then poured into the dish, thus forming an effectual seal, and sterilised water was poured on to the surface of the mercury. The weight of the bell-jar causes it to sink to a certain depth into the mercury, so that the damp-chamber is in reality cut off from the external air by the mercury, and not by the sterilised water. A piece of sterilised india-rubber tubing is then introduced beneath the mercury, and a current of any particular gas can be passed into the chamber, the excess of gas escaping at the edge of the bell-jar through the mercury and water.

After the air has been driven out of the chamber in this manner, and replaced by any given gas, the tubing is removed, and the dish is kept at the requisite temperature, which in my experiments was about 20° C.

The particular micro-organisms which I used in these experiments were (1) the *Bacillus pyocyaneus*, (2) *Koch's Comma Spirillum*, (3) *Finkler's Comma Spirillum*, which were procured from the Hygienic Institute in Berlin. The different organisms were obtained in a suitable degree of attenuation by mixing them with sterilised water, from which a definite quantity was taken and gelatine plates poured.

In each experiment one plate was placed in a damp-chamber containing ordinary air, whilst a second was exposed in a similar chamber filled with the particular gas under examination. After the lapse of an adequate period of time admitting of their development, the colonies were counted in both cases and the results compared.

I. *Experiments with Hydrogen.*

The hydrogen was generated in a Kipp's apparatus by the action of dilute sulphuric acid on zinc; it was purified by passing it through a saturated solution of caustic soda, and was then conveyed through a sterilised piece of india-rubber tubing and a sterile plug of cotton-wool into the damp-chamber containing the inoculated gelatine plates. The following results were obtained in the use of this gas:—

(*a*.) With *B. pyocyaneus* (Green Pus).

1st Experiment (March 4th, 1886).

	Air-plates (after 2 days).		H-plates (after 4 days).
Number of colonies from 1 c.c. of the mixture..	(*a*.) 22,412	22,500	11,500
	(*b*.) 22,651		

The appearance presented by the plates developed in the hydrogen-chamber and those developed in air was very different. On the former the colonies were decidedly larger, less sharply defined, fainter in colour, and of more radiated structure than those on the air-plates.

2nd Experiment (March 11th, 1886).

	Air-plates (after 5 days).		H-plates (after 7 days).	
Number of colonies from 1 c.c. of the mixture..	(*a*.) 15,515	17,200	(*a*′.) 12,365	12,300
	(*b*.) 18,950		(*b*′.) 12,262	

The hydrogen-plates again showed the characteristic appearances mentioned above, many of the surface colonies having reached a diameter of 1 cm.

3rd Experiment (March 29th, 1886).

	Air-plates (after 4 days).	H-plates (after 7 days).
Number of colonies from 1 c.c. of the mixture............	6124	5600

In this case again the hydrogen-plates had the same characteristic appearance, the colonies on the surface being surrounded by a complete circular zone which exceeded by many diameters the original size of the colony.

From these experiments it is seen that the development of the *Bacillus pyocyaneus* is only slightly affected in an atmosphere of hydrogen; the colonies, however, grow more slowly and present a distinctly different appearance.

(*b*.) With Koch's *Comma Spirillum*.

1st Experiment (March 15th, 1886).

	Air-plates.	H-plates (after 7 days).
Number of colonies from 1 c.c. of the mixture ..	(*a*.) 4183 (after 4 days)	(*a*′.) 6767
	(*b*.) 4440 (after 5 days)	(*b*′.) 8260

The colonies on the hydrogen-plates were smaller than those on the

air-plates, and they did not exhibit the characteristic depression on the surface of the gelatine.

2nd Experiment (March 29th, 1886).

	Air-plates (after 4 days).	H-plates (after 7 days).
Number of colonies from 1 c.c. of the mixture............	100	110

The vitality of Koch's comma spirillum is therefore in no way affected by exposure to an atmosphere of hydrogen, although its development into colonies is considerably retarded.

(*c*.) With Finkler's *Spirillum*.

1st Experiment (March 15th, 1886).

	Air-plates (after 4 days).	H-plates (after 7 days).
Number of colonies from 1 c.c. of the mixture............	12,107	6726

The colonies on the hydrogen-plates had the appearance of small milky dots, and caused in many cases a depression on the surface of the gelatine; they resembled, in fact, very young colonies on an ordinary plate culture of these spirilla.

In an atmosphere of hydrogen it would appear that of the three organisms with which I have experimented Koch's comma spirilla were the least prejudicially affected in their vitality.

II. *Experiments with Carbonic Anhydride.*

The gas was prepared in a Kipp's apparatus by the action of dilute hydrochloric acid on marble, and purified by passing it first through a saturated solution of carbonate of soda and then through a sterilised plug of cotton-wool.

The same three micro-organisms were submitted to experiment in the manner previously described.

(*a*.) With the *B. pyocyaneus*.

1st Experiment (March 4th, 1886).

	Air-plates (after 2 days).	CO_2-plates (after 9 days).
Number of colonies from 1 c.c. of the mixture............	22,412 22,651	0*

*This plate was then placed in a damp-chamber in an atmosphere of air, and after seven days 2023 colonies were found.

2nd Experiment (March 11th, 1886).

	Air-plates (after 5 days).	CO_2-plates (after 8 days).
Number of colonies from 1 c.c. of the mixture............	(*a.*) 15,515	(*a'.*) 0*
	(*b.*) 18,950	(*b'.*) 0

*On being transferred to a damp-chamber filled with air, there were after three days—

(*a'*) 1288 colonies.
(*b'*) 1150 „

In an atmosphere of carbonic anhydride *B. pyocyaneus* is thus not only prevented from multiplying, but the greater proportion of the bacilli present are destroyed in the course of a few days.

(*b.*) With Koch's *Comma Spirilla* (March 11th, 1886).

	Air-plates	CO_2-plates (after 8 days).
Number of colonies from 1 c.c. of the mixture ..	(*a.*) 4183 (after 4 days)	(*a'.*) 0*
	(*b.*) 44[illegible]0 (after 5 days)	(*b'.*) 0

*These plates were then transferred to a damp-chamber filled with air, and examined after three days, but *no* colonies were found.

(*c.*) With Finkler's *Spirilla* (March 11th, 1886).

	Air-plates (after 4 days).	CO_2-plates (after 8 days).
Number of colonies from 1 c.c. of the mixture	12,107	(*a'.*) 0*
		(*b'.*) 0

*These plates were then transferred to a damp-chamber filled with air, and re-examined after three days, but *no* colonies were found.

The deleterious effect of carbonic anhydride on the vitality of these organisms is, therefore, far more intense in the case of the Koch and Finkler spirilla than in that of the *Bacillus pyocyaneus*, for not only can no colonies develop in the atmosphere of CO_2, but the spirilla are either destroyed or so weakened during eight days' exposure to this gas that even on being transferred to an ordinary air-chamber no colonies are developed.

III. *Experiments with Carbonic Oxide.*

This gas was prepared from potassium ferrocyanide and strong sulphuric acid, and purified by passing it through a saturated solution of caustic soda and then through a small tower filled with slaked lime, and finally through a plug of sterilised cotton-wool.

The following experiments were made in the manner previously described with the three micro-organisms mentioned :—

(*a.*) With *B. pyocyaneus.*

1st Experiment (March 19th, 1886).

	Air-plates (after 4 days).	CO-plates (after 8 days).
Number of colonies from 1 c.c. of the mixture............	(*a.*) 28,952	(*a.'*) 0*
	(*b.*) 27,794	(*b.'*) 0

*After three days' exposure to air, there were found on examination—

(*a'*) 20,558 colonies.
(*b'*) 16,142 „

2nd Experiment (March 29th, 1886).

	Air-plates (after 4 days).	CO-plates (after 9 days).
Number of colonies from 1 c.c. of the mixture............	6124	467*

*After five days' exposure to air, 6333 colonies were found.

3rd Experiment (April 10th, 1886).

	Air-plates (after 4 days).	CO-plates (after 7 days).
Number of colonies from 1 c.c. of the mixture............	113,978	0*

*In this experiment a dish with pyrogallic acid and caustic potash was placed in the damp-chamber, in order to remove any trace of free oxygen which might be present. After four days' subsequent exposure to air, 100,821 colonies were found.

From the above experiments, it is evident that carbonic oxide exerts a very powerful influence on the vitality of *B. pyocyaneus*, for it effectually stops their development, but that this is only a temporary check to their growth is shown by the fact that on being removed to a damp-chamber containing air, almost the same number of colonies made their appearance as were found in the first instance on the air-exposed plates. The results of the 2nd experiment suggest that in this case there were possibly traces of air still remaining in the chamber.

(*b.*) With Koch's *Comma Spirilla.*

1st Experiment (March 29th, 1886).

	Air-plates (after 4 days).	CO-plates (after 9 days).
Number of colonies from 1 c.c. of the mixture............	100	48*

*After five days' exposure to the air, the number of colonies rose to 76.

2nd Experiment (April 10th, 1886).

	Air-plates (after 4 days).	CO-plates (after 7 days).
Number of colonies from 1 c.c. of the mixture........ ...	(a.) 2,800 (b.) 52,020 (c.) 52,470	(a′.) 809 (b′.) 19,494*

*In these experiments pyrogallic acid was employed. The plates were exposed afterwards during four days to the air, but on subsequent examination the number of colonies was not found to have increased.

(*c.*) With Finkler's *Spirilla* (April 10th, 1886).

	Air-plates (after 3 days).	CO-plates (after 7 days).
Number of colonies from 1 c.c. of the mixture	(a.) 4574 (b.) 4320	2*

*In this experiment pyrogallic acid was employed. After four days' exposure to the air, the number of colonies rose to 501.

In the carbonic oxide atmosphere, therefore, only a fraction of Koch's comma spirilla, and a still smaller fraction of Finkler's spirilla are developed; the subsequent growth on exposure to the air is relatively small, and in the case of Koch's comma spirilla practically *nil.*

IV. *Experiments with Nitrous Oxide, Nitric Oxide, Sulphuretted Hydrogen, and Sulphurous Anhydride.*

Similar experiments were made with these gases. Those plates which were exposed to an atmosphere of nitric oxide, sulphuretted hydrogen, or sulphurous anhydride developed no colonies, neither were any found on subsequently placing the plates in air-chambers. These three micro-organisms are, therefore, rapidly destroyed by the action of these gases.

In the experiments with nitric oxide, the air was first driven out of the damp-chamber with hydrogen in order to prevent the formation of nitrous acid.

The organisms behaved, however, differently in the presence of nitrous oxide; in the chambers which were filled with this gas, and in which pyrogallic acid was also present, the *Bacillus pyocyaneus* developed no colonies, but afterwards on being placed in an air-chamber, almost as many colonies were found as were present in the original control air-plates.

Under similar circumstances, Koch's comma spirilla developed in an atmosphere of nitrous oxide nearly one-third of the colonies

found on the control air-plates, and on being transferred to the air-chamber a further though slight increase was found on re-examination.

In the case of the Finkler spirilla, about one-seventh of the total number of colonies were developed, and on being transferred to the air-chamber a further increase was observed, being about one-fifth of the total number which had grown on the control air-plate.

These results are tabulated below.

Experiments with Nitrous Oxide.

This gas was prepared by heating ammonium nitrate in a retort, and purified by passing it through a small tower filled with slaked lime, also through strong sulphuric acid and sterilised cotton-wool. In all the experiments, a dish containing pyrogallic acid and caustic potash was placed in the damp-chamber.

(*a*.) With *B. pyocyaneus* (April 10th, 1886).

	Air-plate (after 4 days).	N_2O-plate (after 7 days).
Number of colonies from 1 c.c. of the mixture	113,978	0*

*On being transferred to an air-chamber, there were found after four days 89,368 colonies.

(*b*.) With Koch's *Comma Spirilla* (April 10th, 1886).

	Air-plates (after 4 days).	N_2O-plates (after 7 days).
Number of colonies from 1 c.c. of the mixture............	(*a*.) 2,800	(*a'*.) 903*
	(*b*.) 52,020	(*b'*.) 17,496
	(*c*.) 52,470	

*On being placed in an air-chamber no further colonies were developed on (*a'*) plate, whilst on (*b'*) after four days the number had risen to 23,328.

(*c*.) With Finkler's *Spirilla* (April 10th, 1886).

	Air-plates (after 3 days).	N_2O-plates (after 7 days).
Number of colonies from 1 c.c. of the mixture............	(*a*.) 4574	649*
	(*b*.) 4320	

*On being transferred to an air-chamber there were found, after two days, 816 colonies.

Nitrous oxide acts, therefore, upon these three micro-organisms much in the same manner as carbonic oxide.

Remarks.

From the above series of experiments, it is at once apparent that the four different gases act very differently towards micro-organisms. Of the four gases employed, hydrogen, carbonic oxide, nitrous oxide, and carbonic anhydride, hydrogen had the least deleterious effect upon those microbes with which I experimented, whilst carbonic anhydride had the most destructive influence. There is, therefore, no longer any doubt, as indeed Liborious and C. Fränkel have already pointed out, that in the anaërobic culture of micro-organisms hydrogen is by far the most suitable medium for the expulsion of air, whilst carbonic anhydride, owing to its markedly deleterious effect upon many forms of bacteria, is not only ill suited, but is in many cases quite unfit for such a purpose.

And although there is no doubt, as Buchner asserts, that all those bacteria which give rise to fermentations attended with an abundant evolution of carbonic anhydride, must also be capable of flourishing in an atmosphere of this gas, yet it by no means follows that these organisms attain their full vitality in such an atmosphere. On the contrary, it is very possible that their anaërobic and fermenting powers only reach their maximum degree of activity when the gaseous products to which they give rise are removed either by a really indifferent gas, such as hydrogen, or by a vacuum.

The results of some experiments on the fermentative activity of yeast by Boussingault ('Compt. Rend.,' vol. 91, p. 37) support this view, for they show that in such a vacuum alcoholic fermentation takes place more actively, and is more quickly completed than at the ordinary pressure of the atmosphere.

As regards the particular behaviour of these three micro-organisms towards carbonic anhydride, the results of my experiments agree almost entirely with those of Fränkel. In both series of experiments it was found that the growth of *B. pyocyaneus* was entirely suspended by the action of this gas, but that on subsequent exposure to air the growth, attended with the formation of the characteristic pigment, commenced.

Again, in both series of experiments, it was observed that carbonic anhydride completely arrested the growth both of Koch's comma spirillum and Finkler's spirillum, but whilst C. Fränkel always succeeded on subsequent exposure to the air in obtaining a growth, although a very feeble one, in my experiments no such secondary growth was observed.

This discrepancy may, however, very possibly arise from the difference in the power of resistance which is often observed in the same

organism in different cultures. Of particular interest is the fact, which is brought out in the quantitative results of the experiments made by both of us, that there is a great variation in the power of resistance possessed by the individual organisms in an ordinary cultivation, and that conditions which exert a rapidly destructive influence on the majority of the microbes, leave the more hardy individuals of the same culture unaffected.

I have already had occasion* to notice a similar result in experiments on the introduction of Koch's comma spirilla and *B. pyocyaneus* into drinking water; in these experiments it was repeatedly observed that the greater proportion of the organisms which were inoculated into the water rapidly died off, whilst a small proportion survived much longer, and, in fact, subsequently exhibited multiplication.

II. "The Spinal Curvature in an Aboriginal Australian." By D. J. CUNNINGHAM, M.D., Trinity College, Dublin. Communicated by Sir W. TURNER, Knt., F.R.S. Received January 14, 1889.

(Abstract.)

1. The lumbo-vertebral index gives no information as to the character and degree of the lumbar curve of the vertebral column. If it did so, we might assume that in the native Australian the lumbar region of the spine was curved so as to present a concavity to the front.

2. To estimate the extent and the degree of the different curves of the column it is necessary to examine fresh spines in which both the vertebral bodies and intervertebral disks may be studied in conjunction with each other.

3. In the spine of the native Australian (described in the extended paper) the secondary curves (*i.e.*, the cervical and the lumbar curves) are strongly accentuated, whilst the primary curves (*i.e.*, dorsal and sacral) are not so marked. In these particulars the Australian spine resembles somewhat the spine of a Chimpanzee.

4. The points of inflexion of the axial curvature of the vertebral column, in the case of the cervico-dorsal transition and the dorso-lumbar transition, are placed differently in the Australian from the corresponding points in the European female and the Chimpanzee

5. In the European the sacral curve is cut off in the most decided manner from the lumbar curve: not so in the Australian. In the latter the first sacral vertebra just escapes being included in the lumbar curve, and the importance of this is centred in the fact that

* "On the Multiplication of Micro-organisms." 'Roy. Soc. Proc.,' vol. 40, 1886, p. 543.

in the Chimpanzee the lumbar curve passes continuously into the sacral region, and involves one, or perhaps two, of its vertebræ.

In connexion with this question, it is interesting to note the close association which the last lumbar vertebra in the Chimpanzee exhibits with the sacrum. The intervening disk of cartilage is very thin, and quite different from those above it. Further, it is extremely common to find the last lumbar vertebra of the Chimpanzee presenting sacral characters and joined by osseous union to the sacrum. In the Australian and European the last lumbar and first sacral vertebræ are well separated from each other by a thick pad of intervertebral substance, but there is reason to believe that the last lumbar vertebra of the Australian more frequently exhibits sacral characters than the corresponding vertebra of the European.

6. A single glance at the tracing obtained from the mesial section of the Australian spine will be sufficient to dissipate any doubt that may be remaining regarding the presence of a lumbar convexity in the vertebral column of this race. Not only does it exist, but it exists in a very pronounced form. The degree of curvature in the lumbar region of the Australian, while it falls slightly short of that which is seen in the Chimpanzee, corresponds closely with the degree of curvature in the European female. At the same time we must not lose sight of the fact that the lumbar curve does not consolidate until adult life, and the Australian spine was taken from a girl who had only reached the age of sixteen. It is more than probable, therefore, that the spine in question does not express the full amount of curvature of the lumbar region in this race.

7. In the Australian, the curvature in the lumbar region is entirely due to the strongly wedge-shaped form of the intervertebral disks. If we form an index for these, as has been done for the vertebral bodies, the amount which they contribute to the curve can be appreciated. The following are the indices obtained for the Australian spine, and also the average indices of the spines of four Europeans :—

	Lumbo-vertebral index.	Lumbo-intervertebral index.
Australian..........	101·6	49·5
Four Europeans	96·3	65·6

8. In the extended paper the character of the lumbar axial curve is discussed, and it is shown to differ materially from that of the European. In both it is composed of the arcs of three circles. The parts entering into these, and the lengths of their respective radii differ in the two cases markedly. In the Australian the lower part of the curve is abrupt and sudden; in the European it is more uniform throughout.

9. A sagitto-vertical index of the lumbar vertebræ suggests some

interesting points. In calculating this, the sagittal diameter of the different vertebral bodies is taken as the standard and compared with the axial vertical diameter. This index is observed to present a direct relation to the proportion of bone and cartilage which enters into the construction of the lumbar column. The higher the index the smaller is the amount of intervertebral substance, and *vice versâ*. This has been tested in the European, Australian, Chimpanzee, Baboon, Macaque, and Orang. The European excels all these in the amount of cartilage as compared with bone in the lumbar region of the vertebral column. In the erect attitude of man this greater amount of cartilage lessens the shocks transmitted upwards through the column. In the prone or semi-prone position of the trunk the same provision is not so necessary.*

Presents, January 24, 1889.

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* The spine of the aboriginal Australian referred to in the foregoing abstract was obtained from Professor T. P. Anderson Stuart, of Sydney University.

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January 31, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Paper was read:—

I. "On *Isoëtes lacustris*, Linn." By J. BRETLAND FARMER, B.A., F.L.S. Communicated by Professor S. H. VINES, F.R.S. Received January 22, 1889.

The genus *Isoëtes* has been an object of interest to botanists ever since Hofmeister's brilliant researches on the vascular cryptogams, but the accounts given by the different observers on the development and organogeny of the sporophyte are so conflicting, and moreover our knowledge of the sexual generation is so limited, that a renewed investigation of the whole subject seems desirable. In the present communication I propose to summarise, as briefly as possible, the more important of my own observations on one species, *I. lacustris*, to which plant my attention has been directed for some time past. I intend to deal here only with the germination of the macrospore, and to reserve details of minor significance, as well as all account of the sporophyte, for treatment in a future paper, as this part of the subject requires critical discussion.

The shape of each macrospore is, as is well known, that of a tetrahedron with somewhat rounded sides, and the protoplasmic contents are enclosed in a number of coats which, in mature specimens, are differentiated into six layers. Peripherally is the episporium, a colourless, glassy, and brittle layer, whose surface is beset with numerous irregular prominences. The episporium, which is derived from the epiplasm of the sporangium, stains with hæmatoxylin, though only to a slight extent. Within this outer layer is the exosporium, consisting of three brown cuticularised layers, but of which the two outer ones are frequently not easily distinguishable as separate coats. The two innermost membranes of all, are cellulose in character, and form the endosporium.

The protoplasm which is contained in the spore includes a large quantity of reserve material, consisting of starch and oil, the latter being, however, eliminated during the process of soaking in turpentine, to which the spores are subjected previously to their being

embedded in paraffin. A number of sections through each spore were obtained by means of the Cambridge rocking microtome, and were arranged in series, thus permitting of an examination of the internal structure of the spores. The protoplasm, which is remarkably granular, is of a spongy texture (probably due to the extraction of the oil), and contains a nucleus of very large size, in which bodies resembling nucleoli were in some cases detected. The nucleus is sharply marked off from the cytoplasm by a membrane, but of course it must be borne in mind that this feature may be caused in part by the methods used in embedding. When spores are examined in this stage, the protoplasm stains but slightly with hæmatoxylin, and the tint is inclined to red, and even the nucleus is not deeply coloured. In somewhat older spores, at the period immediately preceding germination, the whole protoplasm stains far more readily and deeply in a given time, but a nucleus is no longer differentiated by the hæmatoxylin, and the colour now produced is of a deep blue. As I have frequently had spores of different ages on the same slide, all of which were subjected to exactly similar treatment, this difference in colour may probably be taken to indicate an actual diffusion of the substance of the nucleus through the cytoplasm, since the change is always confined to spores in the condition referred to.

This view receives some confirmation from the circumstances attending the formation of the prothallium, now to be described. The first indication of cell-division occurs in a somewhat peculiar manner, but its significance is rendered clear by what takes place subsequently. Before entering upon a description of what actually happens, it may be well, in order to avoid possible misconstruction, to state expressly the opinion that the characters presented are made visible only by the action of the means necessarily employed in embedding, but this does not vitiate the conclusion that they may be taken as indications of internal changes which actually occur in the protoplasm. In spores in which cell formation is about to commence, the deeply stained protoplasm is seen to be traversed by a few "cracks," which divide the contents of the spore into large isolated masses. At this period there is nothing to point to the existence of a membrane, except the granular structure which is apparent on the surface of the cracks, but at a subsequent stage in the development, one of the surfaces is seen to be bounded by a membrane of extreme tenuity. When first formed it can only be distinguished in favourable places, but it rapidly grows in thickness, and forms a limiting surface between the two protoplasmic masses. From the mode of its formation it can hardly arise otherwise than by the conversion of a layer already present in the protoplasm directly into cellulose, and it appears to be the presence of this substance arranged in a definite plate-like manner which determines the splitting referred to. The first membrane cuts

the spore into an apical and a basal portion, and while the latter for some time undergoes no further change, the apical cell is divided very rapidly into a number of cells, whose arrangement can still be followed even in quite old prothallia. When the first primary cells are formed, the nuclei are again distinguishable on staining with hæmatoxylin, but they are of exceedingly small dimensions, and with this change the staining capacity of the protoplasm becomes less marked. Divisions in all planes proceed very rapidly in the upper (apical) portion of the prothallium, and the rudiments of the archegonia are laid down much as in the Marattiaceæ. Periclinal division of single superficial cells into two takes place, the upper of which gives rise to the neck, and by repeated division forms four stories, each story being again divided crosswise into four cells arranged as quadrants of a cylinder. The lower cells form the central series, in which a neck canal cell is cut off, and then a ventral canal cell, from the oosphere. The canal cells then thrust themselves between the neck cells, and cause a distortion in the two lower stories, which may be so great as even to render them difficult of recognition.

Whilst these changes have been taking place in the upper (apical) of the two primary cells, the lower (basal) one is dividing, but comparatively slowly, and it is easily distinguishable in that the cells arising from it remain of a large size as compared with those formed in the upper part of the prothallium. In spite of repeated search through a great number of preparations, it has not been found possible hitherto to arrive at a satisfactory conclusion as to the mode of cell-division which prevails in the secondary stage, for no karyokinetic figures could be detected; nevertheless, it is highly probable that the process does not differ in any important respect from that exhibited by other plants, and the arrangement of the nuclei about the walls of recently formed cells makes this supposition almost a certainty.

I have purposely omitted any reference to the researches of other observers in the present paper, and it was not my object to attempt a complete account of my own work, which is still in progress, but the results detailed above appeared of sufficient interest to justify the appearance of this note.

II. "On Auto-infection in Cardiac Disease." By L. C. WOOLDRIDGE, M.D., D.Sc., Assistant Physician to Guy's Hospital. Communicated by Professor VICTOR HORSLEY, B.S., F.R.S. (from the Laboratory of the Brown Institution). Received January 24, 1889.

In 1886 I described to the Royal Society* a substance, one of the most noticeable features of which was that it caused intravascular clotting when injected into the circulation of an animal. In subsequent publications I have further described the action of this substance, or rather group of allied substances, and speak of them as fibrinogens.

In particular, I pointed out in my papers in du Bois-Reymond's 'Archiv,' 1886, and in Ludwig's 'Festschrift,' 1887, that the lymph and chyle contained this substance. More exactly I had found that the fluid of lymphatic glands, freed from all form elements, possessed precisely the same action as the fibrinogens, and that the fibrinogen was the active substance in this fluid. The lymph contained in serous cavities does not contain this body, hence it is probably formed in the lymphatic glands. Dr. Krüger,† assistant to Professor Alexander Schmidt of Dorpat, has disputed the correctness of these observations. But I am absolutely certain, from a repetition of my experiments, an account of which I have published elsewhere,‡ that Dr. Krüger is in error, and that my original observations were correct.

In the present paper I endeavour to show the light which further experiments have thrown on this question, and to point out the probably great importance which fibrinogen intoxication plays in a large and important class of disease, particularly cardiac disease.

For the purpose of my experiments I have used mainly the thymus gland, as the fluid and the fibrinogen of the thymus is quite similar to that of lymphatic glands, and is more easily obtained.

Experiment 1.

The half per cent. NaCl fluid of the thymus, perfectly fresh, the cells completely removed by the centrifuge. The fluid rendered faintly alkaline with Na_2CO_3.

Dog I.—Weight 19 lbs. Injected rapidly into the jugular vein 8 c.c. of the fluid. Dog killed. The portal vein was thrombosed,

* "On Intravascular Clotting," and Croonian Lecture Abstract, Apr. 8, 1886.

† Krüger, 'Zeitschrift für Biologie,' 1887, Heft 2.

‡ 'On the Nature of Coagulation' (pamphlet, London, 1888).

the clot commencing in the middle of the portal trunk, and extending into all the branches of the portal in the liver.

Dog II.—Weight 16 lbs. 7·5 c.c. of the fluid injected, but ten times diluted with alkaline salt solution. The injection was slow, taking from three to four minutes. The dog was killed. There was absolutely no trace of clotting in any vessel.

As regards diet the animals were in similar conditions.

Experiment 2.

Used the watery extract of thymus, precipitated with acetic acid, and the solution of this precipitate in alkaline half per cent. NaCl injected.

Dog I.—Weight of dog, 14 lbs. Injected rapidly 7 c.c. of solution. The animal ceased to breathe instantly and never breathed again. The heart continued to beat for several minutes. The right heart, the whole of the pulmonary artery and veins, and the left heart one solid clot.

Dog II.—Weight of dog, 13½ lbs. 7 c.c. of the same solution injected, but diluted ten times with alkaline salt solution; the injection slow, occupying three to four minutes. Dog killed. Absolutely no trace of clotting anywhere.

It is seen from the above experiments that a substance added rapidly to the circulating blood produces a pronounced effect; added comparatively slowly and diluted, but in the same quantity proportionate to the weight of the animal, it produces no effect at all.

The obvious effect may be local, *i.e.*, occur where the sudden admixture of fluids takes place, *i.e.*, in the heart; or it may be remote and take place in the portal vein.

The phenomenon appears to resemble somewhat the so-called "mass influence" (*massenwirkung*) of chemists.

A sudden admixture of a sufficient quantity of this substance with a given quantity of blood poisons the blood; the same conditions would be produced if instead of the injection being sudden the blood were circulating more slowly. In this case, also, a given quantity of the blood would in a given time receive a larger quantity of the fluid than if the blood were rapidly circulating. For the present I am speaking of the blood being affected by its showing an obvious change, that is clotting; and I know, from previous experiments, that to produce this change a certain quantity of the fibrinogen must be added to the blood, *i.e.*, the larger the dog, and consequently the more blood, the more of fibrinogen must be injected.

The present experiments show that to affect the blood a certain quantity of the substance must reach the blood within a given time, and this effect may obviously be obtained either by rapid injection or by the current of blood being slow in the neighbourhood of the vessel

used for injection. I am therefore inclined to explain the fact that the lymph does not normally poison the blood because it runs into the blood slowly whilst the blood circulates rapidly. In a normal state, therefore, the conditions which must exist for a fibrinogen intoxication do not prevail.

I have above used the term "poison the blood"; it will be advantageous for me to explain this expression.

The admixture of fibrinogen and blood may obviously affect the latter, by causing it to clot or by preventing its clotting (*vide* previous papers),* but it produces other changes than these which are not so directly perceptible. The nature of these changes will be seen from the following:—If in a normal dog the femoral vein be ligatured there is no obvious effect produced, *i.e.*, there is no œdema of the leg. If, however, some solution of fibrinogen be injected into the circulation through the jugular vein and the femoral be then ligatured, the effect produced is most pronounced, and is as follows: either the most extensive and rapidly developing simple œdema of the leg occurs or an enormous hæmorrhage "*per diapedesin*" takes place throughout the tissues of the limb; or the two are combined—there is hæmorrhage and œdema.

The injection of fibrinogen,† then, in addition to the obvious effects of clotting or delay in clotting, produces a totally disturbed relationship between the blood and the vascular wall, since, after the injection, a slight mechanical disturbance to the circulation causes a greatly increased exudation of the fluid of the blood, or this associated with a free passage of the red corpuscles. The tendency the injection has to cause hæmorrhage I have already pointed out in a previous publication,‡ the fact that it produces a simple but severe and sudden œdema is new. Now, to produce this altered state of the blood, leading to œdema, the same conditions of admixture of blood and fibrinogen are necessary, *i.e.*, the admixture must be rapid. I will illustrate this by an experiment.

Experiment 3.

Used the NaCl fluid of thymus free from cells.

Dog I.—Weight 17 lbs. 12 c.c. of solution rapidly injected into the jugular. Right femoral vein tied close to Poupart ligament. Dog killed the next day. The portal system thrombosed. The whole

* "Intravascular Clotting," 'Roy. Soc. Proc.,' 1886; "Beiträge zur Frage der Gerinnung," 'du Bois-Reymond, Archiv,' 1888; "Ueber Schutzimpfung auf Chemischem Wege," 'du Bois-Reymond, Archiv,' 1888.

† The fibrinogen used to produce this effect may be lymph fibrinogen, tissue fibrinogen, or certain varieties of blood fibrinogen.

‡ Wooldridge, "On Hæmorrhagic Infarction of the Liver," 'Pathol. Soc. Proc.,' 1888.

right leg extremely œdematous. Large hæmorrhages over upper part of leg and lower part of abdomen.

Dog II.—17 lbs. 12 c.c. of solution injected, but ten times diluted, and injection lasting five minutes. Femoral vein tied close to ligament. Dog killed next day. No trace whatever of clotting anywhere. Leg absolutely free from the slightest trace of œdema or hæmorrhage.

So far as my observations go, the tendency to œdema is the first symptom of fibrinogen intoxication, *i.e.*, it is more easily produced than any other.

One of the most important features in these observations lies in their relationship to many important diseases. I have pointed out the conditions which must prevail to produce a fibrinogen intoxication. It is improbable that diseased conditions are often set up by a sudden large flow of lymph into the blood; but it is certain that the other conditions, the slowing of the circulation in the neighbourhood of the thoracic duct, is a common incident, particularly I may mention valvular disease of the heart and obstruction to the circulation through the lungs, as conditions which necessarily produce this result. It is a dogma of medicine that cardiac dropsy as a symptom of cardiac failure, is due to the mechanical obstruction of the circulation. My observations lead me to the conclusion that the danger in cardiac disease is fibrinogen intoxication; and that the symptoms of cardiac disease—*e.g.*, dropsy, formation of intravascular clots, hæmorrhagic infarction, fever, &c.—are largely dependent on this condition.

Presents, January 31, 1889.

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February 7, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Second Series of Results of the Harmonic Analysis of Tidal Observations." Collected by G. H. DARWIN, LL.D., F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge. Received January 18, 1889.

[Publication deferred.]

II. "The Principles of training Rivers through Tidal Estuaries, as illustrated by Investigations into the Methods of improving the Navigation Channels of the Estuary of the Seine." By LEVESON FRANCIS VERNON-HARCOURT, M.A., M.Inst.C.E. Communicated by A. G. VERNON-HARCOURT, F.R.S. Received January 19, 1889.

[Publication deferred.]

III. "Note on the Spectrum of the Rings of Saturn." By J. NORMAN LOCKYER, F.R.S. Received and read February 7, 1889.

The acknowledged meteoritic constitution of the rings of Saturn rendered it important to obtain a photograph of their spectrum, in order that it might be determined whether collisions there were of sufficient intensity to produce incandescent vapours. It has long been known that the rings appear much more luminous than the planet, and the magnificent photographs obtained by the Brothers Henry show that this is truer for the blue light than for the visual rays.

The weather has been so bad that only one long exposure photograph has been taken. Although the instrument was not in perfect

adjustment, owing to a recent accident, I submit it to the Society because there appears to be evidence of bright lines in the photograph. It is altogether too early to announce this as an established fact, but I think it well to send in this note, in order that other observers with more powerful optical appliances and a better climate than that of London may investigate the question.

The photograph exhibited was taken on the 2nd instant by Mr. Porter, Computer to the Solar Physics Committee. The instrument employed was the 10-inch equatorial of the Science Schools, and a spectroscope of two prisms of 60°.

Other considerations point to the possibility that bright lines or bands may be found in the spectrum of Uranus.

Presents, February 7, 1889.

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Amsterdam:—Koninklijke Akademie van Wetenschappen. Jaarboek. 1886–87. 8vo. *Amsterdam*; Verslagen en Mededeelingen (Letterkunde). Deel IV. 8vo. *Amsterdam* 1887. Ditto (Natuurkunde). Deel III–IV. 8vo. *Amsterdam* 1887–88; Catalogus der Verzamelingen Bilderdijk en Van Lennep. 8vo. *Amsterdam* 1887. The Academy.

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February 14, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Magnetisation of Iron at High Temperatures." (Preliminary Notice.) By J. HOPKINSON, F.R.S. Received January 30, 1889.

I have recently been making some determinations of the curves of magnetisation of iron at varying temperatures up to that at which the iron ceases to be magnetic. Although the experiments are still progressing, some of the results are of sufficient interest to be worth publishing briefly at once.

The method of experiment was identical with that which I used for a sample of nickel about a year ago. The temperatures are estimated by the resistance of a copper secondary coil, and as there may be some uncertainty as to what temperatures the several resistances correspond with, I give in the curves which follow the resistance observed as well as the temperature estimated.

Curve I shows the relation of induction to magnetising force at the ordinary temperature, the resistance of the secondary coil being 0·692 ohm. The curve is given to two scales, the scale of induction being the same in each, whilst the scale of magnetising force is magnified twenty-fold in the one as compared with the other.

Curve II shows the same relation for a temperature of 697° C. to 700° C.

Curve III shows the same thing for a temperature between 727° C. and 720° C.

These curves illustrate what has been long known, that rise of temperature causes increase of induction if the magnetising force is small, but diminution of induction if the force is great.

In Curve IV the abscissæ are temperatures, the ordinates are the ratios of induction to magnetising force or permeabilities for a force of 4·0, and of 0·3 C.G.S. units, the data being supplied from the preceding and other curves. The latter curve brings out a most remarkable feature. For this force the permeability increases somewhat steadily to a temperature of about 640° C., its rate of increase then rapidly accelerates, till it attains a maximum of 11,000 at a temperature of 727° C.; at 737° C. the permeability is practically unity, or the magnetisability of the material has entirely disappeared.

Regarding the iron as made up of magnetic molecules the axes of which are directed to parallelism by magnetic forces, the results are expressed by saying that the magnetic moment of the molecule diminishes with rise of temperature, at first slowly, but very rapidly as the point is approached at which magnetism disappears; on the other hand, the facility with which the particles are directed continually increases, at first slowly, but at high temperatures very rapidly. The effect is that at a temperature of 720° C. an exceedingly small force is competent to turn the axes of nearly all the molecules in a direction parallel to the magnetising force.

The estimates of temperature given herein must be accepted as provisional, and subject to revision. The actual temperatures are undoubtedly materially higher, as I have not yet taken into account the part of the secondary wire outside the furnace.

[If an iron ring which has never been magnetised has its curve of magnetisation determined for an ascending series of forces, if it be then thoroughly demagnetised by a succession of reversed currents of descending intensity, and the curve of magnetisation is redetermined, I find that the two curves differ materially. The demagnetising currents do not reduce the iron to its virgin state. For small forces the second curve is below the first, indicating less induction for the same magnetising force; for medium forces the second curve is above the first, whilst for large forces the two curves agree.

If a ring be heated with a current through the primary coil and the heating be continued till the ring has ceased to be magnetic, if then the current be stopped and the ring be allowed to cool, I find that the ring is not entirely demagnetised by the heating, but that it recollects its state of magnetisation before heating. It would seem that the magnetic molecules of the iron, having been directed by the magnetising force whilst they were magnetic, retain in part their direction when they have ceased to be magnetic by heating, and that when they again become magnetic by cooling its effect is apparent.—February 14, 1889.]

I have tested a sample of manganese steel, and find that at no temperature above the normal temperature does it become substantially magnetic.

II. "On a Series of Salts of a Base containing Chromium and Urea.—No. 2." By W. J. SELL, M.A., F.I.C. With Crystallographic Determinations by Professor W. J. LEWIS, Cambridge. Communicated by Professor G. D. LIVEING, F.R.S. Received February 1, 1889.

In a former paper ('Roy. Soc. Proc.,' vol. 33, 1882) a number of salts were described derived from a base formed by a combination of chromium with urea. It was stated that the chief product of the regulated action of chromyl dichloride on dry urea, and subsequent treatment with water, is a green crystalline powder, insoluble in alcohol, ether, or chloroform. The compound thus obtained contains chlorine as an essential component, while, as noted, the product of crystallisation from hot water is the pure dichromate of the base. At the date of the previous publication the nature of this green salt, as a preliminary to the study of the reaction by which it is produced, was under investigation. The present paper deals with these subjects, and describes a number of additional salts of the base.

The failure in the attempt to purify the green salt by crystallisation from water, added to its insolubility in all other available neutral

menstrua, rendered it advisable to make some preliminary analytical determinations on the different samples of crude well-washed substance. From the results obtained it was evident that the salt was either a chlorochromate of chromium urea, or a compound of the dichromate and chloride, a conclusion which at once harmonises with its genesis, and suggests the trial of dilute hydrochloric acid as a possible vehicle for its purification by crystallisation. The purification by dilute hydrochloric acid containing one volume of strong acid to nine of water, was successful, the numbers obtained on analysis being substantially the same as those obtained from the crude well-washed product of the reaction. The analytical results were satisfied by either of the formulæ—

$$(1.)\quad \{(CON^2H^4)^{12}Cr^2\}{(Cr^2O^7)^2 \atop Cl^2}2H^2O,$$

or

$$(2.)\quad \{(CON^2H^4)^{12}Cr^2\}{2CrO^4 \atop 2CrO^3Cl}2H^2O.$$

The latter, however, is regarded as very improbable.

It is difficult to believe that a chlorochromate in fine powder can withstand the action of water for weeks without appreciable change. The fact also that the hydrochloric acid used in its recrystallisation may be replaced by metallic chlorides, such as those of sodium or potassium, is against the second formula. Moreover, the decomposition effected when the salt is recrystallised from water, may be cited. A chlorochromate having the formula given in (2) should normally decompose, according to the equation—

$$\{(CON^2H^4)^{12}Cr_2\}{2CrO^4 \atop 2CrO^3Cl} + H^2O = 2HCl + \{(CON^2H^4)^{12}Cr^2\}{2CrO^4 \atop Cr^2O^7},$$

whereas the decomposition effected by water is of a totally different character, and may be represented thus:—

$$3\{(CON^2H^4)^{12}Cr^2\}{(Cr^2O^7)^2 \atop Cl^2} = 2\{(CON^2H^4)^{12}Cr^2\}(Cr^2O^7)^3 + \{(CON^2H^4)^{12}Cr^2\}Cl^6.$$

These facts may be regarded as conclusive that formula (1) is the more correct representation of this compound, which may be called—

Dichlordichromate of Chromium Urea.

The following results were obtained on analysis:—

The samples employed were dried by pressure between bibulous paper.

Theory.			Analysis.									
		Percentage.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
C^{12}	144·0	10·54	10·55									
H^{48}	48·0	3·80	4·00									
N^{24}	336·0	24·60	..	24·61								
O^{14}	224·0	..										
Cr^{2}	104·8	7·67	..	..	..	..	..	..	..	..	..	7·56
$(CrO^{3})^{4}$	401·6	29·41	..	..	29·74	29·77	29·70					
Cl^{2}	71·0	5·19	..	..	..	..	..	5·21	5·35			
$2H^{2}O$	36·0	2·63	..	..	..	..	..	..	..	2·69	2·61	
	1365·4											

1. 0·4658 gram gave on combustion 0·18025 gram CO^2 and 0·168 gram water.
2. A modification of Liebig's process gave equal volumes of CO^2 and N.
3. 0·4005 gram precipitated by mercurous nitrate gave 0·0906 gram Cr^2O^3.
4. 0·5294 gram, treated as in 3, gave 0·1199 gram Cr^2O^3.
5. 0·4925 gram dissolved in H^2O, excess of KI and HCl added, and the iodine titrated with thiosulphate, required 44·6 c.c.; each c.c. thiosulphate = 0·00328 gram CrO^3.
6. 0·374 gram reduced by sulphurous acid, excess of latter expelled by heat, $AgNO^3$ added, and the whole strongly acidified with HNO^3, gave 0·0789 gram AgCl.
7. 0·4554 gram, treated as in 6, gave 0·09853 gram AgCl.
8. 0·42415 gram lost *in vacuo* over H^2SO^4 0·0114 gram H^2O.
9. 0·3243 gram lost at 100° C. 0·00845 gram H^2O.
10. Two separate experiments gave 33·68 per cent. Cr^2O^3 on ignition. Deducting Cr found as CrO^3, gives 7·56 per cent. Cr.

The crystals have a distinct oblique habit, but are very ill developed, and few of the planes are truly parallel, or in their true zones. The measurements and elements are, therefore, but approximations. They were obtained from six of the best crystals I could find. The crystals are dark green and have a fairly good cleavage, $n(\bar{1}01)$, perpendicular to the plane of symmetry.

The system is oblique, and the elements are (100, 101) = 49° 2′; (010, 111) = 45° 6′; (101,001) = 35° 20′.

The planes observed are $a(100)$, $l(101)$, $c(001)$, $n(\bar{1}01)$, and $m(110)$. The planes l are generally those most largely developed.

	Calculated.	Observed means.	Extremes.
	° ′	° ′	° ′ ° ′
al	49 2	49 10	48 35—50 17
ac	84 22	84 13	82 14—84 51
cn	39 30½	39 20	
na_1	56 7½	55 59	55 34½—56 22
am	59 45	59 37	58 46—61 19
mm_1	60 30	59 51	57 55—60 33
ml	70 43	70 0	
mc	92 50	92 44	
m_1n	73 41½	73 1½	

The question naturally arises, "Is the compound last considered the initial chief product of the reaction of chromyl dichloride on urea, or is it produced from this compound by interaction with the water added subsequently? As at the present time difficulties, which seem insurmountable (see below), attend the direct determination of this question, and as, moreover, it was suspected that the salt above described is the product of the action of water on the chlorochromate of the base, it was determined before proceeding further to attempt the isolation of such compounds. With this object in view an investigation was made of the action of aqueous hydrochloric acid on the compound last considered. As mentioned above, the dichlordichromate crystallises out unaltered from a hot solution containing one volume of strong acid to nine of water. If, however, the quantity of acid to water be increased to one in six, a salt crystallises out as the solution cools in brownish-yellow crusts of small crystals. When a much stronger acid than one in six is used, the product is a mixture of the brown-yellow salt and green needles of the chloride. The examination of this brown-yellow compound showed it to be the dichlortetrachlorochromate of the base having the composition—

$$\{(CON^2H^4)^{12}Cr^2\}{}^{4CrO^3Cl}_{Cl^2}3H^2O,$$

formed from the dichlordichromate by the following change:—

$$\{(CON^2H^4)\}^{12}Cr^2{}^{2Cr^2O^7}_{Cl^2}+4HCl=\{(CON^2H^4)^{12}Cr^2\}{}^{4CrO^3Cl}_{Cl^2}+2H^2O.$$

The colour of the new salt presents a striking contrast to that of the preceding compound. With the exception of the acid of the strength from which it has been crystallised, it is either insoluble in, or decomposed by, all the usual solvents. With alcohol the chloride of the base is formed, and the usual products of the action of chromic acid on that reagent. Water effects immediate decomposition, the colour changing to the characteristic dark green of

the dichlordichromate, hydrochloric acid being at the same time set free, thus :—

$$\{(CON^2H^4)^{12}Cr^2\}{}^{4CrO^3Cl}_{Cl^2} + 2H^2O = \{(CON^2H^4)^{12}Cr^2\}{}^{2Cr^2O^7}_{Cl^2} + 4HCl.$$

It is extremely probable that this salt is the chief initial product of the reaction between chromyl dichloride and urea, and that the subsequent addition of water decomposes it, as shown by the preceding equation. Granting that a chlorochromate is formed (and as the reaction takes place in presence of excess of chromyl dichloride, it is difficult to resist this conclusion), the only salt of this character which would normally decompose by water with production of the dichlordichromate, is the compound under consideration.

The following results were obtained on analysis. The numbers refer to the dry salt, unless stated to the contrary :—

1. 0·4736 gram salt gave 0·28333 gram AgCl.
2. 0·7059 „ „ when precipitated with mercurous nitrate and the precipitate ignited, 0·14928 gram Cr^2O^3.
3. 0·3938 gram salt, dissolved in dilute HCl, excess of KI added, and the iodine titrated with thiosulphate, required 33·4 c.c.; each c.c. thiosulphate = 0·0032459 CrO^3.
4. 0·14235 gram, treated as in 3, required 12·2 c.c. same thiosulphate.
5. 0·4054 gram moistened with alcohol, dried and ignited, gave 0·12925 gram Cr^2O^3. Deducting from this the Cr existing as CrO^3, gives 10·74 Cr^2O^3, or 7·000 per cent. Cr.
6. 1·1153 gram undried salt lost *in vacuo* over sulphuric acid 0·03885 gram H^2O.
7. 0·7766 gram undried salt lost at 100° C. 0·0236 gram H^2O.

Theory.			Analysis.						
		Percentage.	1.	2.	3.	4.	5.	6.	7.
C^{12} ……	144·0								
H^{48} ……	48·0								
N^{24} ……	336·0								
O^{12} ……	192·0								
Cr^2 ……	104·8	7·28	..	..	..	..	7·00		
$(CrO^3)^4$ …	401·6	27·90	..	27·68	27·53	27·63			
Cl^6 ……	213·0	14·79	14·78						
	1439·4								
$3H^2O$ ….	54·0	3·61	..	..	..	..	..	3·48	3·63
	1493·4								

The direct decision of such an apparently simple matter as the composition of the chief initial product of the reaction has, up to the present time, been found to be impossible. No reagent or mixture of reagents has been discovered which at once dissolves any excess of materials used and the other products of the reaction, without producing some change in the composition of the chief product.

A careful examination of the reaction between chromyl dichloride and urea was made by taking known weights of the materials, collecting and measuring the gaseous products, and after the addition of water to the residue, estimating the dichlordichromate produced, as well as the other products which pass into solution. The dichlordichromate was then calculated as dichlortetrachlorochromate. Without going into a mass of detail, it may be stated that the results of the examination gave numbers very nearly agreeing with the equation—

$$13CO(NH^2)^2 + 9CrO^2Cl^2 = \{(CON^2H^4)^{12}Cr^2\}{}^{4CrO^3Cl}_{Cl^2} + Cr^2Cl^6$$

$$+ H^2CrO^4 + H^2O + CO^2 + N^2 + 3Cl^2.$$

With regard to the preparation of these substances, it may be well to note that the reaction of chromyl dichloride on urea succeeds best in narrow test-tubes, working with about 3 grams of urea. On a larger scale the reaction becomes very difficult to control, and decomposition more or less complete is very liable to ensue. On the other hand, unless the reaction is fairly vigorous and the temperature allowed to rise, little or none of the compound is produced.

A considerable amount of time has been taken up in attempts to prepare this class of compounds by some modification of the above process which should present less complexity, and thus offer some hope of arriving at their constitution. Passing over the unsuccessful attempts, it was discovered that the dichromate of the base may be obtained by the action of chromic acid on urea. In the month of September of last year three separate portions of nearly equal weights of urea and chromic anhydride were dissolved in a small quantity of cold water, the solutions covered with filter-paper, and allowed to stand at the ordinary temperature.

On examination in March the solutions had changed colour, become quite thick from evaporation, and on addition of water a small quantity of sparingly soluble green crystals were found to be left. These, when separated and recrystallised from hot water, had all the characters of the dichromate of the base, and gave on ignition 41·32 per cent. of Cr^2O^3, against 41·43 as required by theory for the dichromate. It was subsequently found that the dichromate may be formed in some quantity by evaporating the aqueous solutions of the mixed

substances at about 60° C. The nature of this change is at present under investigation.

In addition to the foregoing, the following new compounds have been examined:—

The Chromate.

This compound separates from a warm saturated solution of the dichromate, cautiously neutralised with ammonium carbonate, in long dark-green needles. The crystals are very efflorescent, and rapidly become opaque from loss of water. They are sparingly soluble in cold, more readily in hot water, undergoing at the same time slight decomposition, with separation of brown flocks of chromic chromate. The salt is insoluble in alcohol, ether, carbon disulphide, and benzene, and has the composition $(CON^2H^4)^{12}Cr^23CrO^44H^2O$.

The following results were obtained on analysis. The salt was dried by pressure between bibulous paper:—

1. 0·3274 gram salt lost *in vacuo* 0·0196 gram H^2O.
2. 0·3274 „ ignited left 0·0994 gram Cr^2O^3 = 30·36 per cent. Deducting Cr existing as CrO^3, leaves 11·94 Cr^2O^3, or 8·18 per cent. Cr.
3. 0·5465 gram salt lost at 100° C. 0·03315 gram H^2O.
4. 0·5465 „ ignited gave 0·16585 gram Cr^2O^3 = 30·34 per cent. Subtracting Cr existing as CrO^3 leaves 11·92 Cr^2O^3, or 8·17 per cent. Cr.
5. 0·4774 gram salt, dissolved in dilute HCl, excess of KI added, and the iodine estimated by thiosulphate, required 35·62 c.c.; each c.c. thiosulphate = 0·0032459 CrO^3.

Theory.		Analysis.				
	Percentage.	1.	2.	3.	4.	5.
CrO^3	24·17	..	..	..	..	24·21
Basic Cr.........	8·41	..	8·18	..	8·17	
H^2O	5·77	5·98	..	6·06		

The Bromide.

This compound is conveniently prepared from the dichlorchromate by first forming the very soluble acetate by double decomposition with lead acetate, filtering off the mixture of lead chromate and chloride, and precipitating the bromide from the filtrate by dissolving in it crystals of potassium bromide. The drained and washed precipi-

tate, recrystallised from warm water, separates in bright green prismatic crystals containing 6 mols. of water of crystallisation. The salt is tolerably soluble in cold, freely in hot water, insoluble in strong solutions of alkaline bromides, and in the usual organic menstrua. It has the composition $\{(CON^2H^4)^{12}Cr^2\}Br^6 6H^2O$. The following results were obtained on analysis:—

1. 1·403 gram lost *in vacuo* 0·108 gram, and no further loss was sustained at 101° C.
2. 1·18515 gram dissolved in water, the Cr separated by boiling for some time with precipitated chalk, and after filtration the filtrate made up to 250 c.c. Mean of four concordant titrations with $AgNO^3$ required 10·17 c.c.; each c.c. $AgNO^3$ = 0·0035293 Cl.

Theory.		Analysis.	
	Percentage.	1.	2.
H^2O	7·61	7·62	
Br.............	33·97	..	34·11

Dark-green crystals, in which the $f\{3\bar{1}\bar{1}\}$ planes were most prominently developed. The planes $s\{\bar{1}11\}$ and $a\{10\bar{1}\}$ were about equally developed; and the planes $b\{2\bar{1}\bar{1}\}$ and $r\{100\}$ were all small, and these latter did not seem to be present on all the crystals. The habit of the crystal rendered it a little puzzling to decipher the symmetry by inspection.

The principal zones measured were those containing poles *a*, *f*, *s*, and these angles were alone depended on in determining the element.

	Calculated.	Observed.
$af =$	41° 2¼′	41° 2½′, mean of 14 observations.
$fs =$	48 57¾	48 58 „ 12 „
$bf =$	29 26	Approximations to these angles were obtained on a somewhat altered crystal.
$fr =$	36 40	
$rs =$	65 27	
$sb =$	48 27	

$D = or = 23\ 54$

No cleavage was perceived.

The Iodide.

This salt was prepared from the dichlordichromate by precisely the same method as the bromide, only that potassium iodide replaced the bromide. It crystallises from water in long brilliant green prisms, free from water of crystallisation. It is insoluble in the usual organic solvents.

The compound has the composition $(CON^2H^4)^{12}Cr^2I^6$, and gives the following results on analysis:—

1. 0·750 gram salt lost no appreciable quantity of water *in vacuo* or at 104° C., and is therefore anhydrous.
2. 1·18 gram salt dissolved in water and made up to 250 c.c. The mean of four concordant titrations with silver nitrate on portions of 50 c.c. each required 8·95 c.c.; each c.c. $AgNO^3$ = 0·0035293 Cl.

	Percentage calculated.	Percentage found.
I	48·02	47·88

The crystals are of a brilliant green colour, in long prisms terminated by rhombohedral planes, often unequally developed.

The forms observed were $a\{10\bar{1}\}$ well developed, $b\{2\bar{1}\bar{1}\}$ very minute, and $r\{100\}$.

The element $D = or$ was found by calculation to be 24° 30¼′.

	Calculated.	Observed.
$ar =$	68° 57′	68° 58′, mean of 3 measurements.
$rr_1 =$	42 6	42 5½ „ 3 „
$aa_1 =$	60 0	59 59½ „ 6 „

The Ferricyanide.

This compound is precipitated in olive-green needles on the addition of potassium ferricyanide to a soluble salt of the base. The salt is sparingly soluble in cold, more readily in hot water, from which it crystallises in long prismatic crystals, having the composition $(CON^2H^4)^{12}Cr^22FeC^6N^68H^2O$.

The following results were obtained on analysis :—

1. 0·3501 gram gave *in vacuo* 0·0351 gram H^2O.
2. 0·3501 „ „ on ignition 0·0790 gram $Cr^2O^3+Fe^2O^3$.
3. 0·3418 „ „ at 100° C. 0·0343 gram H^2O.
4. 0·3418 „ „ on ignition 0·0777 gram $Cr^2O^3+Fe^2O_3$.

Theory.		Analysis.			
	Percentage.	1.	2.	3.	4.
H^2O	10·33	10·02	..	10·04	
Cr^2O^3 + Fe^2O^4	22·46	..	22·56	..	22·73

The Ferrocyanide.

This compound is precipitated in green needles, when a soluble ferrocyanide is mixed with a soluble salt of the base. The crystals are very sparingly soluble in water, either hot or cold, and insoluble in the usual organic solvents.

The examination of the substance led to the formula

$$\{(CON^2H^4)^{12}Cr^2\}^23FeC^6N^617H^2O$$

being assigned to it.

The following results were obtained on analysis :—

1. 0·2396 gram salt gave *in vacuo* 0·0279 gram H^2O.
2. 0·2396 „ „ on ignition 0·0504 gram $Cr^2O^3+Fe^2O^3$.
3. 0·4387 „ „ *in vacuo* 0·0524 gram H^2O.
4. 0·4387 „ „ on ignition 0·094 gram $Cr^2O^3+Fe^2O^3$.

Theory.		Analysis.			
	Percentage.	1.	2.	3.	4.
H^2O	11·80	11·64	..	11·90	
Cr^2O^4 + Fe^2O^3	21·05	..	21·03	..	21·42

The Picrate.

The addition of an aqueous solution of picric acid to a solution of any of the salts of the base produces an immediate separation of the picrate in the form of beautiful green-yellow needles. The salt dissolves readily in alcohol, sparingly in benzene and water, and is practically insoluble in chloroform. The compound recrystallised from water has the composition—

$$(CON^2H^4)^{12}Cr^2(C^6H^2(NO_2)^3O)^6 8H^2O.$$

The following determinations were made:—

	Found (per cent.).	Calculated (per cent.).
H^2O	$\left\{\begin{matrix} 4\cdot95 \\ 5\cdot07 \end{matrix}\right\}$	5·04
Cr^2O^3	5·35	5·34

Double Salt of the Chloride with Mercuric Chloride.

When solutions of the chloride of chromium urea and mercuric chloride are mixed, a beautiful pale-green crystalline precipitate is produced, consisting of micaceous scales. The compound is very sparingly soluble in cold, very moderately in hot water, and insoluble in the usual organic solvents. The crystals are anhydrous, and may be represented by the formula $(CON^2H^4)^{12}Cr^2Cl^6 6HgCl^2$.

The following results were obtained on analysis:—

1. 1·7738 gram salt ignited with lime gave 0·789 gram metallic mercury.
2. 2·542 gram salt ignited with lime gave 1·1339 gram metallic mercury.
3. 0·599 gram salt ignited alone gave 0·0352 gram Cr^2O^3.
4. 0·9321 " " " 0·0553 " "

Theory.		Analysis.			
	Percentage.	1.	2.	3.	4.
Hg	45·04	44·49	44·6		
Cr^2O^3	5·73	..	..	5·97	5·93

Double Salts of Oxalate of the Base with Chromium Oxalate No. 1.

In attempting to prepare the oxalate of chromium urea from the acetate by the addition of a cold saturated solution of ammonic

oxalate, there was slowly deposited during several days a quantity of very dark-green, almost black, crystals with exceedingly bright faces. The crystals on examination were, however, found to be a double oxalate of the base with chromium oxalate, having the formula $(CON^2H^4)^{12}Cr^2(C^2O^4)^3Cr^2(C^2O^4)^3 4H^2O$.

They are very sparingly soluble in cold, more readily in hot water, and insoluble in the usual organic solvents.

The following results were obtained on analysis:—

1. 0·4217 gram gave 0·0187 gram H^2O.
2. 0·4217 " " on ignition 0·0818 gram Cr^2O^3.
3. 0·14165 " " on combustion 25·06 c.c. N and 48·27 c.c. CO^2 at 0° C. and 760 mm.

Theory.		Analysis.		
	Percentage.	1.	2.	3.
Carbon	18·82	..	..	18·31
Nitrogen	21·96	..	..	22·19
Water	4·70	4·43		
Cr^2O^3	19·38	..	19·37	

This substance crystallises in the rhombic system, and has a well-marked hemihedrism with inclined faces. The crystals consist of well-developed prisms with a large deeply striated brachypinakoid, terminated sometimes by six planes, sometimes by four equally developed planes, and sometimes by two prominent planes of κ (111), with other minor planes. The form (210) is also present, but the planes of this form are dull and deeply striated. The prism planes are also sometimes considerably striated, but the striations on m and m_1 on the same crystal or on the parallel faces do not as a rule correspond. The development of the crystals is to a certain extent shown by the accompanying diagrams, figs. 1 and 2, which represent some of the crystals measured by me. The prism in fig. 2 is placed horizontally for showing the hemi-pyramids more distinctly.

FIG. 1.

FIG 2.

The forms found are a 100, l 210, m 110, p $\kappa(111)$, n 101, r $\kappa(101)$.

The elements are :—

$$010, 011 = 59^\circ\ 51' ;\ 001, 101 = 22^\circ\ 58' ;\ 100, 110 = 53^\circ\ 43' ;$$

or $$a : b : c = 1{\cdot}3705 : 1 : 0{\cdot}580843.$$

The angles observed are compared in the following table with those calculated from the elements :—

	Calculated.	Observed.
am	53° 53′	
mm	72 14	72° 18½′
lm	19 27¾	
m_1p........	54 17	54 13½
pp_1	71 26	71 27½

	Calculated.	Observed.
	° ′	° ′
an	67 2	67 1
nn_1	45 56	45 50½
┌ rn	46 55½	46 48½
│ np	28 8	28 14½
└ rp	75 3½	75 3
rr_1	102 4½	102 12
┌ mn	76 42·2	76 46
│ nr_1	61 38·3	61 48
└ r_1m	41 39·5	41 28
┌ l_1n_1	71 13·4	
│ n_1p	52 10·3	52 6
└ pl_1	56 36·3	56 42
pr_1	40 14·5	40 21·5

The striations on the planes a and l were parallel to their intersections, and rendered the readings obtained from them in the zone [alm] valueless, except for the sake of identification. No distinct cleavage was observed.

No. 2.

The foregoing experiment having failed to give the pure oxalate, recourse was had to the decomposition of the pure chloride with silver oxalate. The two substances warmed together with water for some time and filtered gave an abundant crop of dark-green crystals belonging to the anorthic system. Examination showed, however, that the salt differed from the preceding one only in containing more water of crystallisation, and that it readily parts with the latter even in a corked tube, becoming less soluble and possibly forming the preceding compound. This substance, which has the composition $(CON^2H^4)^{12}Cr^2(C^2O^4)^3Cr^2(Cr^2O^4)^3 29H^2O$, gave the following results on analysis :—

1. 0·5113 gram lost *in vacuo* 0·1347 gram H^2O, and suffered no further loss at 100° C. The dry salt ignited left 0·0788 gram Cr^2O^3.

	Calculated.	Found.
H^2O in 100 parts	26·36	26·34
Cr^2O^3 „	15·43	15·41

The crystals are dark-green in colour, and have bright and for the most part well-developed faces. They seem to have no good cleavage. They crystallise in the anorthic system, and the zones [mn], [$cbdl$] are those most largely developed, and give the habit of the crystals.

The planes m, n, and c, though, as a rule, much more largely developed than any others, were somewhat imperfect and often gave several images. Hence it has been necessary to combine all the observations in order to obtain satisfactory elements. The following elements were ultimately selected, as those which agreed best with the observations. From these elements the axial constants commonly used by Continental crystallographers have been determined, and they and a table of computed and observed angles are subjoined :—

Fig. 3.

Forms observed (fig. 3) :—$a\{100\}$, $m\{110\}$, $n\{\bar{1}10\}$, $x\{3\bar{1}0\}$?, $c\{001\}$, $l\{01\bar{1}\}$, $b\{012\}$, $d\{021\}$, $p\{312\}$, $q\{3\bar{1}1\}$, $f\{31\bar{1}\}$, $g\{3\bar{1}\bar{2}\}$, $h\{31\bar{4}\}$.

Elements :—(100, 110) 38° 33¼′; (110, 010) 32° 2¾′;
(010, 011) 40° 29′; (011, 001) 43° 5½′;
(001, 101) 35° 27′; (101, 100) 40° 21′.

or $A = 88° 5⅝′$; $B = 77° 10′$; $C = 71° 33′$,

and $a : b : c = 1{\cdot}20406 : 1 : 1{\cdot}0238$.

	Calculated.		Observed (means).			Calculated.		Observed (means).	
	°	′	°	′		°	′	°	′
ma	38	33¼	38	36	mf	26	27½	26	30
mx	61	31			fg	47	26½	47	24
mn_1	99	37⅝	99	27	mg	73	53½		
mn	80	22¾	80	29¼	md	34	45½	34	57
010,m	32	2¾							
					bm	56	8½	56	5
cb	26	16⅝	26	21	mh	61	18	61	16½
bd	33	8½	33	8	hb_1	62	33½	62	37
cd	59	25	59	25					
dl	70	48½	70	48	lf	59	34½	59	40½
lc_1	49	51	49	52	la	85	38	85	43½
d,010	24	9			fq	58	49¾	58	36
					ql_1	66	36	66	34½
cn_1	84	48·4	84	56					

	Calculated.		Observed (means).			Calculated.		Observed (means).	
	°	′	°	′		°	′	°	′
c_1h	43	50	43	51	lh	38	21⅓	38	29
hf	42	0	41	53	lg	74	27¼		
c_1f	85	50	85	44	gn_1	50	49⅓	50	56
cp	48	8½	48	22	nl	54	43⅓	54	35¼
nd	57	24	57	26	ca	75	48	75	57
pq	34	38	34	40½					
qn_1	44	22	44	21	hd	89	32	89	42½
qd_1	101	46	101	47	hd_1	90	28	90	19⅓
cm	77	45¾	77	54				58	11¾
mc_1	102	14¼	102	6	cq	58	10	or	
								58	28
pm	34	52½	34	19	qg	62	10⅓	62	1
ml	60	11⅓	60	15	gc_1	59	39⅓	59	39
pl_1	85	0	85	11					

The Periodide.

When a warm solution of iodine in potassium iodide is added to a warm and moderately concentrated solution of the normal iodide, a considerable crop of crystals separates out on cooling in transparent brown-red micaceous scales. If, however, the solutions are heated to near the boiling point before mixing, or are more dilute, especially if the quantity of iodine added is small, the crystals deposited are in the form of long opaque black needles, having a well-marked green reflection, which is, to a certain extent, lost on drying. Not unfrequently, however, both forms are deposited from the same solution. The apparent dissimilarity of form and general appearance led to the analysis of the two modifications being conducted separately. The numbers obtained, however, are identical, and lead to the formula $(\mathrm{CON^2H^4})^{12}\mathrm{Cr^2I^66I^2}$, being assigned to this remarkable substance. The difference in appearance of the two forms is possibly due to certain of the faces in one being differently developed to those in the other. Both yield apparently identical crystals when deposited by spontaneous evaporation from alcoholic solutions, or from the nearly boiling solution in aqueous potassium iodide.

The substance dissolves freely in alcohol, very sparingly in benzene, and is scarcely affected by chloroform, only just sufficient being dissolved to communicate a violet colour.

The Periodide (Red-brown Transparent Six-sided Plates).

1. 0·1849 gram salt leaves on ignition 0·00895 gram $\mathrm{Cr^2O^3}$.
2. 0·2703 gram salt, dissolved in dilute sulphurous acid, warmed to

expel excess, the iodine then precipitated by $AgNO^3$, and whole pretty strongly acidified with HNO^3, gave 0·36523 gram AgI.

3. 0·6262 gram salt, treated exactly as in 2, gave 0·84773 gram AgI.

Theory.		Analysis.		
	Percentage.	1.	2.	3.
Cr	3·36	3·31		
I.............	73·48	..	73·005	73·143

The Periodide (Black Long Needles).

1. 0·2655 gram leaves on ignition 0·01305 gram Cr^2O^3.
2. 0·3818 gram, dissolved in sulphurous acid, excess of latter expelled by heat, silver nitrate added, and whole acidified with nitric acid, gave 0·5191 gram AgI.
3. 0·1748 gram, treated exactly as in 2, gave 0·23665 gram AgI.

Theory.		Analysis.		
	Percentage.	1.	2.	3.
Cr	3·36	3·36		
I	73·48	..	73·43	73·16

Periodide of Chromium Urea (Crystallised from Alcohol).

The periodide is crystallised for the most part in simple hexagonal prisms terminated by the base. The crystals formed on another occasion had the same habit with the edges of the base terminated by narrow planes p and π. The system is rhombohedral, and one small crystal was observed with several well-developed planes on it. This crystal consists of the forms $o(111)$, $r(100)$, $z(\bar{1}22)$, $p(7\bar{2}\bar{2})$, $\pi(\bar{5}44)$, $b(2\bar{1}\bar{1})$, $a(10\bar{1})$. Badly developed planes were also observed on a few other crystals. They are $(8\bar{1}\bar{1})$, $(9\bar{2}\bar{2})$, and $x(52\bar{1})$. The following table gives the observed angles, as also the angles calculated from the element $D = 33° 38'$.

	Calculated.	Observed.
$bb_{\prime\prime}$	60° 0′	59° 58′
$b_{\prime\prime}b_{\prime}$	„	60 2
$or = oz$	33 38	33 38 (mean of 4 measurements.)
$o(8\bar{1}\bar{1})$	44 56⅓	44 34
$o(9\bar{2}\bar{2})$	55 39⅓	55 49
$op = o\pi$	63 23	63 29½
ob	90 0	90 8½ (mean of 5 measurements.)
rz	32 8½	32 6
ox	29 57	30 5
bx	64 23	65 3
xb	115 37	115 8½
$b_{\prime\prime}x$	64 23	63 38½

No satisfactory cleavage was perceived on the crystals.

The Sulphatoperiodide.

This salt is precipitated in silky yellowish-brown needles when a solution of iodine in potassium iodide is added to a solution of the sulphate or any other salt of the base containing sulphuric acid. It is practically insoluble in cold water, dissolving, however, to a small extent in hot water from which it crystallises on cooling in brown needles. The solvent action of water is not materially affected by the presence of potassium iodide, and it is insoluble in the usual neutral menstrua. On boiling with water the compound is decomposed, iodine to the extent of about two-thirds of the total amount present escaping with the steam. The composition of this remarkable salt would appear to be $(CO.N^2H^4)^{12}Cr^2(SO^4)^2I^2I^4$.

The following results were obtained on analysis:—

1. 0·48315 gram salt gave 0·375 gram silver iodide.
2. 1·05455 „ „ 0·824 „ „ „
3. 1·204 „ „ 0·3248 „ $BaSO^4$.
4. 0·6785 „ „ 0·1843 „ „
5. 0·3282 „ „ 0·02965 „ Cr^2O^3.
6. 0·6503 „ „ distilled with water, the evolved I collected in KI, titrated with thiosulphate, required 12·13 c.c. (each c.c. = 0·014035 gram I). The residual liquor gave 0·17542 gram AgI and 0·00805 gram Ag.

Theory.		Analysis.					
	Percentage.	1.	2.	3.	4.	5.	6.
Total iodine ...	42·83	41·93	42·24	..	..	..	42·32
SO^4	10·78	..	..	11·11	11·18		
Cr^2O^3	8·59	..	..	..	..	9·03	
Iodine expelled by boiling with water..............		..	..	..	..	..	26·18
Iodine remaining in liquor..		..	..	..	..	..	16·14

Carbonatoperiodide No. 1.

When a solution of the normal iodide is mixed with ammonium sesquicarbonate and a solution of iodine in potassium iodide carefully dropped in, a yellowish precipitate is produced consisting of fine silky needles. The crystals are insoluble in water and other neutral solvents, and decomposed by acids with effervescence and separation of free iodine. Examination of this remarkable substance led to the formula $(CON^2H^4)^{12}Cr^2(CO^3)^2I^4$ being assigned to it.

The following results were obtained on analysis:—

The numbers refer to the compound dried *in vacuo* over sulphuric acid.

1. 0·4178 gram gave 0·27695 gram AgI.
2. 0·6570 „ 0·4375 „
3. 0·6158 „ on treatment with HCl 0·036 gram CO^2.
4. 0·5217 „ on ignition 0·0547 gram Cr^2O^3.
5. 0·341 gram dissolved in dilute HCl required 4·15 thiosulphate: each c.c. = 0·0380975 I.
6. 0·198 gram dissolved in dilute HCl required 2·45 c.c. same thiosulphate.

Theory.		Analysis.					
	Percentage.	1.	2.	3.	4.	5.	6.
Iodine (total)..	34·86	35 88	35·98				
CO^2	6·06	..	..	5·84			
Cr	7·22	..	..	..	7·18		
Iodine (liberated by HCl) not required for normal compound	17·43	..	..	..	..	18·89	19·23

Carbonatoperiodide No. 2.

When in the preparation of the preceding compound the quantity of the base has been considerably diminished by precipitation, the further addition of iodine no longer produces a yellowish but a well-marked brown precipitate consisting also of fine needles. The brown colour is not due to admixed periodide, as it is perfectly unaffected by alcohol or aqueous solution of potassium iodide, moreover it was formed in the presence of a considerable excess of ammonium sesquicarbonate.

The crystals are insoluble in all neutral menstrua, and decomposed by hydrochloric acid with effervescence and separation of free iodine.

The analyses are rather unsatisfactory, but point to the formula $(CON^2H^4)^{12}Cr^2(CO^3)^2I^6$.

From the nature of the case it is well nigh impossible to see when the precipitation of one compound ends and the other begins, and there is no doubt that the sample analysed contained some of the preceding compound. A better result would probably have been obtained by adding the dilute solution of the normal iodide to the solution of iodine and ammonium sesquicarbonate, so as to maintain an excess of iodine.

The following results were obtained on analysis:—

The compound was dried *in vacuo* over sulphuric acid.

1. 0·4381 gram salt left on ignition 0·0423 gram Cr^2O^3.
2. 0·650 „ lost on treatment with HCl 0·0343 gram CO^2.
3. 0·423 „ dissolved in dilute sulphurous acid and iodine precipitated with $AgNO^3$ 0·3269 gram AgI.
4. 0·407 gram gave 0·3143 gram AgI.
5. 0·3056 gram dissolved in dilute HCl added.

Theory.		Analysis.				
	Percentage.	1.	2.	3.	4.	5.
Cr.	6·14	6·61				
CO^2	5·27	..	5·27			
Total iodine ...	44·64	..	..	41·74	41·72	
Iodine (liberated by HCl)	29·76	..	..	..	..	25·95

The Perbromide.

When a solution of the normal bromide or any other salt of the base is mixed with bromine-water, or better a solution of bromine in aqueous potassium bromide, a precipitate consisting of large bronze-

yellow plates is produced. This beautiful compound is sparingly soluble in cold, more readily in hot water, especially in presence of alkaline bromides, and crystallises out in large prismatic aggregations; alcohol especially when warm takes up the substance, freely decomposing it and depositing the normal bromide, a similar result being obtained with ether and carbon disulphide, in which, however, it is much less soluble. The crystals rapidly lose bromine on exposure to the air, yielding bright-green pseudomorphs of the normal bromide. A specimen of the compound in the form of micaceous scales exposed for three days over lime gave 36·5 per cent. of bromine, against 36·78 required for the normal salt.

Analysis leads to the conclusion that this substance has a similar composition to that of the periodide, viz., $(CON^2H^4)^{12}Cr^2Br^66Br^2$.

The following determination was made:—

1·2668 gram salt was dissolved in dilute sulphurous acid, and all excess of the latter expelled by heat. The solution was mixed with excess of silver nitrate, and the whole pretty strongly acidified with nitric acid, gave 1·91468 gram AgBr.

	Percentage calculated.	Percentage found.
Br	63·58	64·16

Sulphatoperbromide.

This compound is precipitated in green needles when a solution of any salt of the base is mixed with dilute sulphuric acid and bromine-water added. It is sparingly soluble in water, and loses bromine gradually on exposure to the air. The composition is similar to the sulphatoperiodide, viz.:—

$$(CON^2H^4)^{12}Cr^2(SO^4)^2Br^6.$$

This requires per cent.:—

		Found.
SO^4	12·82	12·97
Br	32·06	34·16

The well-marked crystallisations presented by the substances here described, prove them to be definite compounds. Their empiric formulæ, as derived from analysis, are as to complication such as chemists have been wont to expect only in organic substances; and the rational formulæ provisionally assigned to them would hardly have suggested themselves without the clues afforded by the materials and processes employed in their formation. The examination of the decompositions which they undergo under varied conditions, is a problem little more than touched upon, and the same may be said of the action of chromyl dichloride on substituted ureas, including

thiocarbamide. It is hoped, however, that the work at present in progress on this and kindred points will throw some light on the relation which the chromium bears to the rest of the elements in these complicated compounds.

The remaining crystallographic determinations refer to compounds described in the former paper ('Roy. Soc. Proc.,' vol. 33, p. 267).

Platinum Salt of Chromium Urea.

These crystals are minute prisms of yellowish-green colour, and belong to the rhombohedral system. They are combinations of the forms $(10\bar{1})$, (111), and (100).

	Calculated.	Observed.
aa_1	60° 0′	60° 1′
oa	90 0	90 4½
a_1r	72 44	72 42
rr_1	34 32	34 31
or	20 2·6	20 3

The crystals were too minute to render any experiment for cleavage possible.

Chloride of Chromium Urea.

In emerald-green stout crystals belonging to the rhombohedral system. They are combinations of the hexagonal prisms $(10\bar{1})$ with (111) and (100) (figs. 1 and 2).

Fig. 1.

Fig. 2.

The angles of the prism varied considerably, the range being between 60° 28′ and 59° 19′.

	Calculated.	Observed.
ao	90° 0′	90° 0′
ar	90 0	90 6
ro	42 47	42 47
oe	25 3	
ar	53 58	53 53
rr_1	72 4	72 7½

The crystals seem to have no cleavage.

Nitrate of Chromium Urea.

The crystals are of a dark-green colour, and are only translucent in moderately thin plates. They belong to the oblique system, and have a very perfect and facile cleavage, perpendicular to the plane of symmetry. This cleavage plane, though absent, or at any rate very infrequent, as a natural plane, has been selected as the base. The planes $o(491)$ are much striated, parallel to their intersection with one another, and give very bad reflections. The general habit of the crystals simulates that of a crystal of the rhombohedral system with two rhombohedral forms. The faces $p(212)$ give the best reflections; the faces $q(\bar{2}12)$ are not well developed, and the reflections are indifferent. The accompanying diagrams show the forms present. Fig. 1 is an orthogonal projection on the plane of symmetry.

Fig. 1.

FIG. 2.

$a(100)$, $c(001)$, $b(010)$, $p(212)$, $q(\bar{2}12)$, $o(491)$.

Elements and Angles.

$(100, 101) = 51° \; 47\tfrac{1}{2}'$; $(010, 111) = 43° \; 27\tfrac{2}{3}'$; $(001, 101) = 60° \; 22'$.

$a : b : c = 1·214 : 1 : 1·343$.

	Calculated.	Observed.
	° ′	° ′
$p\bar{p}_1$	55 38	55 41
$q\bar{q}_1$	39 10	39 4
qb	70 25	69 8 approx.
ap	56 50	57 0
pq	86 14	86 23
$p\bar{q}$	93 46	93 45
qa_1	36 $56\tfrac{2}{3}$	36 $53\tfrac{1}{2}$
cp	64 4	64 4
pc_1	115 56	116 4
pq_1	75 41	75 38
$p\bar{q}_1$	104 19	104 28
c_1q_1	40 15	
ao	70 $17\tfrac{1}{2}$	70 6
oa_1	109 $42\tfrac{1}{2}$	110 39
co	93 10	93 42
c_1o	86 50	86 8
qo	85 15	85 46
$o\bar{q}$	94 45	94 15
po	47 27	47 $24\tfrac{1}{2}$
po_1	78 30	from 76 12 to 78° 44′
oa	112 $9\tfrac{1}{2}$	112 2

III. "Effect of Floor-Deafening on the Sanitary Condition of Dwelling Houses." By Miss ETTA JOHNSTONE, University College, Dundee, and THOS. CARNELLEY, Professor of Chemistry in the University of Aberdeen. Communicated by Sir H. ROSCOE, F.R.S. Received February 7, 1889.

"Deafening" is the material which is laid upon boards fitted in between the joists of a floor to prevent the passage of sound into the room below. This material is used largely on the Continent and in many parts of this country, especially in Scotland, and is supposed to consist of a mixture of coarse mortar and smith's ashes, but in general it appears to be of a much more questionable nature, particularly in the case of low-class houses. It is also supposed by some builders to prevent the passage of smell; but houses are known to have been rendered uninhabitable by its presence, the cinders, which form the great bulk of the substance, being more or less contaminated according to the place whence obtained and other attendant circumstances.

With the object therefore of ascertaining whether this material was a serious factor in the pollution and vitiation of the air of dwelling-houses, we undertook the analysis of a number of samples from various classes of houses in Dundee, and the results obtained are recorded in the present paper.

Carnelley, Haldane, and Anderson ('Phil. Trans.,' B., vol. 178 (1887), pp. 61–111) have proved that the number of micro-organisms habitually present in the air of a dwelling-house increases with the age of a building. Indeed, some of the older buildings become perfectly infested with them, as shown not only by the results obtained by the above observers in houses and schools in Dundee, but also by those of Miguel in old and new houses in Paris. Indeed, this floor-deafening when impure would appear to be a remarkably good medium for the propagation of bacteria, other conditions being favourable.

Dr. Emmerich, of Leipzig, some years ago ('Zeitschr. f. Biol.,' 1882) made experiments on the effects of this stuff with regard to the air of rooms, and also analysed numerous samples of pure material, some of which were obtained from new buildings on completion, and some from inhabited houses. He found that on washing the floors of rooms, shutting them up for some time, and then examining the air, there was a great increase of carbonic acid, which must have been due to the putrefaction set up by the moisture on reaching the deafening, as all other known sources of carbonic acid were excluded.

As a result of his investigations, he concluded that "there exists

nowhere in nature, not even in the neighbourhood of human dwellings, a (natural) soil so highly contaminated with nitrogenous organic substances and their decomposition products as the deafening material under the floor of dwelling-rooms."

As some of Emmerich's results appear to have been called in question, and *for the purpose of ascertaining* whether a similar state of matters exists in houses in this country, we obtained samples of deafening from dwellings in different parts of Dundee, through the kindness of Mr. Kinnear, of the Sanitary Department. Some of these were taken from ordinary middle-class houses, others from one-, two-, and three-roomed houses of the poorer class, while two were obtained from houses (in Fish Street, Dundee) about 200 years old, and occupied by the poorest class of artisans. The deafening from the lower class of houses, and especially that from the oldest houses, had a most disgusting and filthy smell. All the houses examined, even those of the better class, had been built and occupied more than twelve years. For analysis the material was passed through a wire sieve of $\frac{1}{10}$-inch mesh, and the percentage of fine dust and coarse lumps noted. The fine dust was bottled, and the following substances determined therein by the usual methods:—(1.) Moisture. (2.) Total combustible matter (exclusive of moisture). (3.) Chlorine. (4.) Nitrogen.

The results are given in the following table:—

Table of Results.

	No.	Percentage in deafening of—		Percentage in fine matter reckoned on total deafening.					Situation of house in Dundee.
		Coarse matter.	Fine matter.	Moisture.	Mineral matter.	Combustible matter.	Chlorine.	Nitrogen.	
Four-roomed houses and upwards.	1	93·51	6·49	1·01	5·14	0·33	none	none	Dalhousie Terrace (bathroom).
	2	79·43	20·57	3·83	15·16	1 58	,,	,,	Magdalene Yard Road (bedroom).
	3	74·46	25·54	1·83	16 79	1·92	,,	,,	Dalhousie Terrace (kitchen).
	4	56·99	33·01	0·57	27·89	4·51	,,	,,	Clarendon Terrace (nursery).
	5	73·36	26·64	0·54	20·01	6·09	,,	,,	Hillside, Newport (bedroom).
	6	50 29	49·71	0·91	42·40	6·37	,,	,,	Clarendon Terrace (bedroom).
	7	63·25	36·75	0·93	27·81	7·95	,,	,,	James Square, Newport (bedroom).
	8	47·15	52·85	1 18	44·01	7·61	,,	,,	Clarendon Terrace (nursery).
	9	54·20	45·80	1·53	42 06	2·24	0·032	0·016	Magdalene Yard Road (bedroom).
	10	43 55	56·45	4·83	47·77	3·85	0·016	0·041	Guthrie House.
	11	57·18	42·82	0·80	34 60	7·34	0·025	0·233	St. Mary's Place.
Average...	..	63·4	36·6	1·63	29·42	4·53	0·006	0·026	
Three-roomed houses.	1	58·41	41·59	0·36	33·76	7·48	0·009	0·019	73, Wilkie's Lane.
	2	65·98	34·02	0·58	27·13	6·30	0 0095	0·025	11, Pennycook Lane.
	3	67·07	32·93	0·83	28 36	3·74	0·018	0·052	13, Kinloch Street.
Average...	..	63·82	36·18	0·59	29·75	5·84	0·012	0·032	

Table of Results—*continued.*

	No.	Percentage in deafening of—		Percentage in fine matter reckoned on total deafening.					Situation of house in Dundee.
		Coarse matter.	Fine matter.	Moisture.	Mineral matter.	Combustible matter.	Chlorine.	Nitrogen	
Two-roomed houses.	1	60·93	39·07	1·28	29·22	8·60	0·080	0·134	3, Stewart Street.
	2	47·49	52·51	0·97	49·46	2·08	0·030	0·161	200, Hilltown.
	3	55·00	45·00	2·78	37·63	4·57	0·038	0·175	21, Ogilvie's Road.
	4	61·13	38·87	2·83	32·58	3·45	0·068	0·173	Bell Street.
	5	14·10	85·90	0·93	75·50	9·50	0·088	0·310	3, Watt Street.
	6	4·27	95·73	0·69	90·72	4·31	0·163	0·307	25, Session Street.
Average...	..	40·48	59·52	1·58	52·52	5·42	0·081	0·209	
One-roomed houses.	1	68·09	31·91	0·67	24·03	7·21	0·026	0·115	69, Hilltown.
	2	34·81	65·14	2·85	36·28	26·01	0·138	0·206	Bell Street.
	3	49·78	50·22	1·43	42·91	5·88	0·058	0·225	91, Hilltown.
	4	33·58	66·42	1·13	56·89	8·40	0·075	0·309	35, Union Street.
	5	23·10	76·90	2·07	63·55	11·16	0·373	0·248	Fish Street.*
	6	15·01	84·99	1·82	71·21	11·83	0·386	0·363	Fish Street.*
	7	22·40	77·60	2·86	60·40	14·23	0·311	0·637	Bell Street.
Average...	..	35·25	64·75	1·83	50·75	12·10	0·195	0·300	

* These houses were about 200 years old, and are now pulled down.

The above results show :—

(1.) That the quality of the deafening, as indicated by the percentage of chlorine, nitrogenous organic matter, and combustible matter, runs strictly parallel with the class of house, being by far the worst in the one-roomed houses, and the best in the largest houses.

(2.) That the deafening employed in ordinary middle-class houses is in almost all cases practically free from nitrogenous organic matter and chlorides, and from any disagreeable smell, so that no objection can be raised to the use of deafening of the quality we have examined in this class of house.

(3.) In the poorer class of houses (of three rooms and under) nitrogenous organic matter and chlorides are always present, the percentage being especially high in the older houses, while in many cases the smell is very objectionable. From this it would appear that the air in such houses may be very seriously polluted by the deafening, and thus give rise to ill-health.

In reference to the above results we may remark : (1.) That the cinders, which form the bulk of the deafening used in better class houses are probably of good quality, owing to their being obtained from a non-contaminated source, whereas in the poorer class of houses inferior materials (and possibly ash-pit refuse, &c.) will doubtless be made to serve for filling up the deafening space. (2.) The carpets in the better class of houses are not usually lifted oftener than twice a year, and of course the floors can only be washed at those times, so that the necessary condition of moisture for the growth of micro-organisms is not present to the same extent as in lower-class houses, while at the same time the carpet will act as a partial filter to micro-organisms arising from the deafening material. In the poorer class of houses, however, everything would seem to favour the contamination of the air from this source. The floor boards are often plain jointed, and simply laid side by side, so that when the floor is washed the water has every facility for trickling down to the material beneath. Further, all the household operations of washing, cooking, nursing, &c., have to be carried out in the one or two apartments, and hence the spilling of dirty water, slops, &c., on the floor, and percolation into the deafening below will be of pretty frequent occurrence. The rooms are often overcrowded, and consequently the air is moist and warm, so that the increase and multiplication of micro-organisms would seem to be inevitable.

It has been shown (Carnelley, Haldane, and Anderson, 'Phil. Trans.,' B. (1887), p. 61) that in passing from many to two- and one-roomed houses the air becomes more and more impure, especially with regard to the number of micro-organisms, whilst the death-rate

largely increases, and the mean age at death diminishes. The results of the present paper show that the sanitary condition of the floor-deafening follows a similar order, thus:—

Dundee.		Houses.			
		Four-roomed and upwards.	Three-roomed.	Two-roomed.	One-roomed.
Vital statistics.	Total population	23,007	22,087	79,825	25,410
	Average number of persons per room	1·3	..	3·4	6·6
	Space per person in cubic feet	1,833	..	240	212
	Death-rate per 1000	12·3	17·2	18·8	21·4
	Mean age at death of all who died	40·0	30·6	21·3	20·9
State of the air.	Carbonic acid (vols. per 1000)	7·7	..	9·9	11·2
	Oxidisable organic matter (O required per million vols. of air)	4·5	..	10·1	15·7
	Total micro-organisms per litre	9·0	..	46·0	60·0
State of floor-deafening.	Coarse matter in deafening per cent...........	63·40	63 82	40·48	35·25
	Fine matter in deafening per cent..............	36·60	36·18	59·52	64·75
	Organic matter per cent...	4·53	5·84	5·42	12·10
	Chlorine per cent.	0·006*	0·012	0·081	0·195
	Nitrogen per cent.	0·026*	0·032	0·209	0·300

The results obtained by the authors referred to above have also shown that the micro-organisms do not come either from the breath (at least in health), nor in large numbers from the outside air, so that it would seem clear that they come from some part of, or material in, the room itself. Though our results are certainly not so marked as those of Dr. Emmerich, they show, nevertheless, quite clearly that the deafening material may be and is in the poorer class of houses a source of contamination of the air of dwellings, in that it furnishes a good and suitable medium for the growth of micro-organisms, and gives off fœtid gases from putrefaction, provided the necessary factors, moisture, warmth, and nitrogenous organic matter, are present.

* Had it not been for the abnormally high results obtained in one of these houses in which the drainage was very defective, these numbers would have been very much lower, viz., 0·004 per cent. Cl and 0·0057 nitrogen. Indeed, eight of the eleven houses examined were quite free from both chlorides and nitrogenous organic matter.

IV. "On the comparative Action of Hydroxylamine and Nitrites upon Blood-pressure." By T. LAUDER BRUNTON, M.D., F.R.S., and T. JESSOPP BOKENHAM. Received February 7, 1889.

This communication forms part of an investigation on which one of us (Brunton) has been engaged for some years past, and in aid of which grants have been received from this Society.*

In this investigation the action of various compound ammonias,† and also of some nitrites,‡ and allied bodies,§ has been examined.

The plan of research required hydroxylamine (NH_3O), forming as it does a link between these two classes of bodies, to be specially examined. The action of this body has recently become a subject of experiment by other workers,|| and it therefore seems advisable to publish now one remarkable relationship between it and nitrites, reserving for a later communication other results of this research. Two of the most striking effects of nitrites are: their power (*a*) to alter the colour of the blood,¶ and (*b*) to lower the pressure of blood within the vessels.**

Both of these properties are also possessed by nitroglycerine,†† and Hay has shown that the effect of this substance is due to the fact that it is decomposed in the blood with evolution of nitrous acid.‡‡

Hydroxylamine is a body in which two affinities of nitrogen are saturated by hydrogen instead of by oxygen as in nitrous acid. Its relation to nitrous acid will be seen by a comparison of their graphic formulæ—

Hydroxylamine.	Nitrous acid.
$\begin{matrix}H\\H\end{matrix}{>}N{-}O{-}H.$	$O{>}N{-}O{-}H.$

* May, 1874, for investigation of the physiological action of ammonia, and others in 1877, 1884, and 1887.

† Brunton and Cash, 'Phil. Trans.,' 1884, p. 197.

‡ Brunton and Greswell. Details not published. *Vide* 'St. Bartholomew's Hospital Reports,' 1876, p. 143, and 'Pharmaceutical Journal,' December 22, 1888, pp. 491 and 495.

§ Brunton and Tait, "Physiological Action of Nitroglycerine," 'St. Bartholomew's Hospital Reports,' 1876, p. 140.

|| Binz, "Toxicologisches über das Hydroxylamin," 'Virchow's Archiv.'

¶ A. Gamgee, 'Phil. Trans.,' 1868, pp. 589—626.

** Gamgee, quoted by Brunton, 'Lancet,' 1867, July 27. Brunton, 'Ludwig's Arbeiten,' 1869.

†† Brunton and Tait, 'St. Bartholomew's Hospital Reports,' 1876, p. 144.

‡‡ Hay, "The Chemical Nature and Physiological Action of Nitroglycerine," 'Practitioner,' June, 1883, vol. 30, p. 429.

It was shown by Raimondo and Bertoni* to have the power of producing a chocolate-brown colour of the blood, of lessening its oxidising power, and of producing a change in its spectrum, changes similar to those observed by Gamgee as consequences of the action of nitrites.† Loew‡ found it to be a powerful protoplasmic poison. From a consideration of its chemical properties, Binz§ was led to think that it must be reckoned amongst the bodies which cause paralysis of cells in the nerve-centres, either by setting free active oxygen or one of the halogens, and his experiments showed the correctness of his hypothesis. Raimondo and Bertoni thought that during the reaction between hydroxylamine and blood nitrous acid was formed, and Binz obtained the reaction of nitrites from the blood of animals poisoned by it.

It therefore seemed probable that it would affect the blood-pressure in a similar way to nitrites, and on testing it we found that it does. On injecting the hydrochlorate of hydroxylamine either into the veins or peritoneal cavity, it produces a fall of blood-pressure almost exactly similar to that produced by nitrite of amyl, as will be seen by a comparison of the accompanying curves, in which the fall of blood-pressure is so much alike that it is almost impossible to tell from a mere inspection of the tracings which is due to hydroxylamine and which to amyl nitrite. As hydroxylamine itself is very unstable, and is readily converted into ammonia, we used the hydrochlorate, which we obtained from Messrs. Hopkin and Williams. As hydroxylamine is made commercially by the reduction of nitrites, it appeared possible that the specimen we employed might be contaminated by nitrites, and that its action upon the blood-pressure might be due to impurity and not to the action of the hydroxylamine itself. On testing the specimen we employed by starch-paste and iodine with acetic, sulphuric or hydrochloric acid we got no reaction, and Messrs. Hopkin and Williams also told us that it gave no reaction with metaphenylene-diamine.

We may therefore regard the specimen as pure, and attribute the fall of blood-pressure to the action of the hydroxylamine hydrochlorate, and not to any impurities contained in it.

* Raimondi and Bertoni, 'Annali Univ. di Med.,' vol. 259, 1882, p. 97. Only known to us by abstract in Virchow and Hirsch's 'Jahresber.' for 1882, 1, pp. 393 and 394.

† Gamgee, 'Phil. Trans.,' 1868.

‡ Loew, 'Archiv f. d. ges. Physiol.,' 1885, vol. 35, p. 516.

§ Binz, *op. cit.*

V. "On the Total Solar Eclipse of August 29, 1886." By Captain L. DARWIN, R.E., ARTHUR SCHUSTER, Ph.D., F.R.S., and E. WALTER MAUNDER. Received January 28, 1889.

A preliminary communication will be found at vol. 42, p. 180. The full report is divided into eleven parts, as follows:—

I. Origin of the Expedition and General Preparations, by Captain Darwin, A. Schuster, and E. W. Maunder.
II. Preparations for the Eclipse at Prickly Point, by Captain Darwin and A. Schuster.
III. Totality at Prickly Point, by Captain Darwin and A. Schuster.
IV. On the Accuracy required in adjusting an Equatorial for Photographic Purposes during a Total Solar Eclipse, by A. Schuster.
V. Results of the Photographic Camera at Prickly Point, by A. Schuster.
VI. The Coronagraph, by Captain Darwin.
VII. The Prismatic Camera, by Captain Darwin.
VIII. The Spectroscopic Cameras at Prickly Point, by A. Schuster.
IX. Photographic Results obtained at Carriacou Island, by E. W. Maunder.
X. Description of the Eclipse and Drawing of the Corona, by Irwin C. Maling.
XI. On the Photographs of the Corona obtained at Prickly Point and Carriacou Island, by W. H. Wesley.

VI. "On the Determination of the Photometric Intensity of the Coronal Light during the Solar Eclipse of August 28—29, 1886." By Capt. W. DE W. ABNEY, C.B., R.E., F.R.S., and T. E. THORPE, F.R.S., Professor of Chemistry in the Normal School of Science, South Kensington. Received February 7, 1889.

[For an abstract of the contents see preliminary communication, vol. 44, p. 392]

Presents, February 14, 1889.

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February 21, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Influence of Bile on the Digestion of Starch. I.—Its Influence on Pancreatic Digestion in the Pig." By SIDNEY MARTIN, M.D. (Lond.), B.Sc., British Medical Association Scholar, and Assistant Physician to the City of London Hospital for Diseases of the Chest, Victoria Park, and DAWSON WILLIAMS, M.D. (Lond.), Assistant Physician to the East London Hospital for Children, Shadwell. Communicated by E. A. SCHÄFER, F.R.S. (from the Physiological Laboratory, University College, London). Received February 1, 1889.

The object of the research is to ascertain what influence, if any, the presence of bile or its constituents has on the progress and result of pancreatic digestion; it includes the investigation of any such influence on the amylolytic, the proteolytic, and the emulsive ferments. The present communication deals only with the first named; our experiments have been done chiefly with the pancreas and bile of the pig, but another series in which these secretions in other animals are being examined is in progress; the effect of the presence of bile on all amylolytic digestion, *ex. gr.*, that of saliva and that of vegetable diastase, is a subject which also seems to be worthy of investigation, and is now receiving our attention. In the present communication we detail the result of our experiments with the bile and pancreatic amylolytic ferment of one animal only—the pig.

The fluid to be digested has been made by boiling pure starch in distilled water and carefully neutralising if necessary. Starch 2 grams, water 100 c.c., has been found a convenient strength. Pig's bile has been used either in the fresh state or after careful drying at a temperature not exceeding 27° C. In the later form it was found more convenient for preserving and for manipulation, as it could be accurately weighed. Glycerine extract of fresh pig's pancreas, and

a commercial pancreatin made from pig's pancreas, and ascertained to be rich in the amylolytic ferment, have been used.

Our earliest experiments indicated that bile had a very notable influence on the pancreatic digestion of starch; it caused a rapid disappearance of the blue reaction of starch with iodine.

Experiment A.—Five tubes, *a*, *b*, *c*, *d*, *e*, each containing 50 c.c. of the starch mixture (2 per cent.). With *c* 2·0 c.c. and with *d* and *e* 8·0 c.c. fresh pig's bile were thoroughly mixed. Equal quantities of glycerine extract of pig's pancreas were then simultaneously added to *b*, *c*, and *d*, and all five tubes were placed in a water-bath at 33° C. The colour reaction of solution of iodine with the two control tubes—*a* which contained the starch mixture alone, and *e* which contained the starch mixture and bile (8 c.c.)—remained unaltered throughout the experiment. The changed colour reaction in the other tubes was watched by mixing a drop of the mixture with iodine solution on a white porcelain plate. The blue reaction in *d* rapidly disappeared, being replaced in less than one minute by a purple and in two minutes by a red colour; the red colour became gradually fainter and had entirely disappeared in ten minutes. In *b* and *c* the blue reaction disappeared more slowly, a purple colour being still obtained at the end of ten minutes; no difference was perceptible in this respect between *b* and *c*, a fact which indicates that the amount of bile present must exceed the proportion added to *c* before any accelerating influence was noticeable.

By using weighed quantities of the dried bile it was proved that a larger proportion of bile caused the blue reaction with iodine to disappear more rapidly than a small proportion.

Experiment B.—Four vessels, *a*, *b*, *d*, *e*, containing the starch mixture 2 per cent. To *b* 0·6 per cent. dried pig's bile, to *d* and *e* 3 per cent. dried pig's bile were added and dissolved; to *a*, *b*, and *d* equal quantities of glycerine extract of pig's pancreas were added, and all the vessels were placed in a water-bath at 33° C.; *d* ceased to give any colour reaction with iodine solution in five minutes; at the same moment the reaction given by *b* was reddish-purple, and by *a* purple; *e* remained unchanged.

This increase of rapidity with increasing proportion of bile was found to hold up to 4 per cent. of dried bile (equivalent probably to at least 30 per cent. fresh bile). Beyond this percentage we have not made experiments; a larger proportion of bile rendered the mixture very thick and interfered with the colour reaction.

It was also ascertained that the amount of sugar, estimated as dextrose, formed under the conditions of Experiments A and B, was greater when bile was present, and increased when the proportion of bile was increased.

Experiment C.—Four vessels, *a*, *b*, *c*, *d*, containing the starch

mixture 2 per cent. To *b* 0·6 per cent. dried pig's bile, and to *c* and *d* 2 per cent. dried pig's bile were added and dissolved; to *a*, *b*, and *c* equal quantities of glycerine extract of pig's pancreas were added and all the vessels were placed in a water-bath at 34° C. The colour reaction with iodine given by *d* was unchanged throughout, but *a*, *b*, and *c* gave a varying colour reaction, and changing most rapidly with *c* and least rapidly with *a*. After remaining in the water-bath for eight minutes the vessels were taken out and their contents boiled, to destroy the ferment, and the amount of dextrose estimated by Fehling's method; *a* contained 0·45 per cent., *b* 0·59 per cent., and *c* 0·74 per cent.

A large number of experiments were performed of which the above are quoted as examples, and the conclusion to which we were led was that digestion of starch by extract of pig's pancreas was hastened in the presence of pig's bile. We next sought to ascertain (1) whether this was a property of the bile solids as a whole, or of one or other constituent; and (2) the nature of this hastening action, whether, that is to say, the bile only hastened the transformation of starch into dextrin, or whether there were also constant increase in the amount of sugar formed.

Firstly, as to whether the effect is to be ascribed to the action of any one constituent of the bile. Pig's bile contains bile salts (chiefly hyoglycocholate of sodium*), bile pigment, cholesterin, soaps, and salts together with mucin. We found that an extract of dried bile made with absolute alcohol retained the power of hastening pancreatic digestion of starch, and finally that it was also possessed by the bile salts. It was found in this case also that the amount of sugar estimated as dextrose was greater as the proportion of bile salts added to the mixture was increased up to 2 per cent., beyond which our experiments have not gone. Thus in one experiment the amount of sugar found after half an hour's digestion (*a*) in a mixture to which 0·6 per cent. of bile salts had been added = 1·03 per cent.; (*b*) in a mixture to which 2·0 per cent. of bile salts had been added = 1·25 per cent.; and (*c*) in a mixture to which no bile salts had been added 1·0 per cent.; a large amount of starch mixture was used in this experiment and 0·8 per cent. pancreatin added.

Secondly, as to the nature of the process, whether the bile hastened the transformation of starch into dextrin, or whether there was also an increase in the amount of sugar; this was found to be a somewhat difficult question to solve. The quantitative estimation of a mixture of starch, dextrin, and sugar, or of dextrin and sugar was found to present many difficulties. The amount of sugar was readily estimated

* Jolin ('Zeits. f. Physiol. Chemie,' vol. 11, p. 417) describes *α*- and *β*-hyoglyco-

as dextrose by Fehling's method, but we are unacquainted with any reagent which will effect the separation of dextrin from starch; they can both, however, be precipitated by absolute alcohol. We have made a quantitative estimation of the relative amounts of starch, dextrin, and sugar by the following method: two equal portions of the starch mixture, 2 per cent., were digested with equal quantities of dried pig's pancreatin,* rich in amylopsin, a certain proportion of bile salts (made from pig's bile) having been previously added to one. Digestion was allowed to proceed in the incubator until the reaction of starch with a solution of iodine had completely, or almost completely disappeared from the vessel to which bile salts had been added. Both mixtures were then rapidly boiled to stop the action. The digested mixture was then poured into a dialyser (made of German sausage paper) and dialysed in running water for four or five days, thymol being added to prevent decomposition (which did not occur); the dextrin, sugar, and most of the salts were thus dialysed away, and the total residue (starch) was estimated by evaporating the dialysed liquid to small bulk and filtering into alcohol. The precipitate was caught on a filter, dried at 100° to 110° C., and weighed. The residue of undigested starch was thus estimated. The proportional amounts of sugar and dextrin were estimated by dialysing the liquids digested under the same conditions as those just described, in distilled water for four days, decomposition being prevented by the daily addition of thymol. Equal quantities of the two dialysates, the one containing sugar and dextrin, the other sugar, dextrin, and bile salts, were evaporated to small bulk, the sugar estimated as dextrose by Fehling's solution, the dextrin by precipitating a measured quantity of each concentrated liquid by absolute alcohol, washing with absolute alcohol to remove bile salts, drying at 100° to 110° C., and weighing.

The results are shown in the following experiments:—

Experiment D.—To one of two flasks containing 200 c.c. of the starch mixture (2 per cent.) 0·6 per cent. bile salts was added; 0·8 per cent. pancreatin was then added to both flasks and the mixture digested at 38° C. for two minutes. The flask containing bile salts then gave no reaction with iodine solution, while that which contained pancreatin alone gave a purple reaction. Both fluids were then dialysed in cold distilled water for four days, decomposition being prevented by the daily addition of thymol. Both dialysates, which were faintly acid and contained no starch, were then evaporated to small bulk, and each divided into two parts for the estimation of sugar and dextrin respectively. The former was estimated as dextrose by Fehling's process, the latter by precipitating under absolute alcohol, filtering, drying at 100—110° C., and weighing. The result was:—

* Prepared by Messrs. Savory and Moore.

	Dextrin.	Sugar.
Fluid to which bile salts had been added as well as pancreatin.....	0·30 gram.	1·315 gram.
Fluid to which pancreatin only was added.......................	0·24168 „	1·042245 „

The addition of bile salts therefore had increased the production of sugar in the proportion 5 : 4, and that of dextrin in like proportion.

Experiment E.—This experiment was conducted with the same proportion of each ingredient and in the same manner, with the exception that the fluids were dialysed in a stream of (tap) water; the total residue, after evaporation and treatment with absolute alcohol in the manner previously described, was estimated by drying and weighing. The residue in the fluid containing bile salts weighed 0·314 gram, in the fluid to which pancreatin alone was added, it weighed 0·517 gram. These residues contained starch and a trace of peptone, but no bile salts nor sugar.

Our *conclusions* may thus be briefly stated:—The effect of fresh and dried bile in hastening the pancreatic digestion of starch in the pig is due to the bile salts; these salts possess the power of increasing the amount not only of dextrin, but of sugar estimated as dextrose.

The authors are not at present in a position to explain this influence of bile salts; the pancreatic solution of starch proceeds more rapidly at first in laboratory experiments, and the retardation after a short interval is very marked. It is possible that the bile salts may favour its continuance by entering into combination with the bodies which have this retarding effect.

II. "The Innervation of the Renal Blood-vessels." By J. Rose Bradford, M.B., D.Sc., George Henry Lewes Student. Communicated by E. A. Schäfer, F.R.S. (from the Physiological Laboratory of University College, London). Received February 1, 1889.

The following work was undertaken in order to map out the origin, course, and nature of the renal nerves more accurately than had hitherto been attempted. It was considered (more especially in the light of Gaskell's well-known work on the sympathetic) important to decide whether the renal and other abdominal vascular nerves were of two kinds, *i.e.*, vaso-constrictor and vaso-dilator, or whether the latter nerves could not be demonstrated to exist. This research was carried out exclusively on the dog, inasmuch as this was the animal used by Gaskell in his work.

The principal conclusions arrived at in this communication will be arranged under the following three headings:—

I. *The Origin and Course of the Vaso-constrictor Nerves.*

II. *The Origin and Course of the Vaso-dilator Nerves.*

III. *The Reflex Phenomena of the Renal Vessels.*

It will be necessary, however, to describe shortly the method employed. The general blood pressure and the volume of the kidney as measured by Roy's oncometer were recorded simultaneously, together with a time tracing and a lever marking the moment and duration of the nerve excitation. In this manner both the general and the local effects of any given stimulation were determined simultaneously. The method of preparation of the nerves was as follows: the roots of the nerves were exposed inside the spinal canal, the posterior roots were then divided inside the dura mater, and the entire nerve outside the dura mater arranged for stimulation with suitable electrodes. In some cases the nerves were cut and ligatured and the distal ends excited. By the use of one or other of these methods, the danger of the exciting current spreading to the cord, and so producing reflex effects, was reduced to a minimum. In many experiments this danger was further eliminated by dividing the cord above the level of the nerves excited.

In this communication a nomenclature is adopted which assumes that the dog has twenty dorso-lumbar vertebræ, of which thirteen are dorsal and seven lumbar. For excitation an ordinary Du Bois coil was used with Helmholtz's modification, and the rate of interruption was varied, as will be mentioned more fully below from fifty per second to one per second.

The anæsthetics used were chloroform and morphia, and after the completion of the necessary operative procedure, the animals were curarised, artificial respiration and anæsthetisation being maintained in the usual manner.

It is well known that, when either the renal nerves or the splanchnic nerves are excited, a contraction of the kidney accompanied by a rise of blood pressure is observed. On exciting the lower dorsal nerves inside the spinal canal the same general facts are observed, provided the posterior roots have been divided and care be taken to prevent the spreading of the exciting current to the cord. Before entering into further detail it is necessary to state that in order to get these effects the rate of excitation must not be slower than five per second. Hence, unless otherwise mentioned, it is to be understood that the rate of stimulation was a rapid one, *i.e.*, fifty per second.

I. *Origin and Course of the Vaso-constrictor Nerves.*

No effects have been observed to follow the excitation of the peripheral end of a divided posterior root. Furthermore, the same

result is seen to follow the stimulation of the divided anterior root, and the stimulation of the entire nerve outside the dura mater after previous section of the posterior root. Hence we may conclude that no efferent vasomotor fibres are contained in the posterior roots.

Excitation of the anterior roots, or of the entire nerve after previous division of the posterior root, is followed by contraction of the kidney and rise of general blood pressure when any nerve from the 6th dorsal to the 2nd lumbar is placed on the electrodes. Excitation of the higher nerves, *e.g.*, the 4th or 5th dorsal, produces but slight effects on the general blood pressure, and in the higher ones still, *i.e.*, the 2nd or 3rd, the accelerator fibres are met with in abundance, and hence a small rise of pressure (due to this cardiac effect) is produced. On the other hand, the 3rd lumbar has in many cases yielded no result on excitation, but occasionally a slight rise of general blood pressure has been observed. So that the 6th dorsal and the 2nd lumbar are practically the limits of the series of nerves, the stimulation of which causes any marked effects either on the kidney or on the general arterial tension.

The effects, however, are not equally marked with all those nerves. The lower dorsal nerves, *i.e.*, from the 10th to the 13th, produce much greater effects, both on the kidney and on the general blood pressure, than either the nerves above them or those immediately below them. So that although all the nerves from the 6th dorsal to the 2nd lumbar may contain fibres for the renal vessels, still their main supply is derived from the 10th, 11th, 12th, and 13th dorsal nerves.

It follows from the above description that there is no very great separation between the paths followed by the nerves for the kidney vessels and those destined for the vessels of the other abdominal viscera. However, the lower dorsal not only produce greater effects on the kidney and on the general blood pressure than the upper dorsal nerves, but what is more important the two effects do not vary directly with one another. Although usually a nerve producing a large renal contraction causes simultaneously a great rise of pressure, yet this is by no means invariably the case, and a small renal contraction may be accompanied by a great rise of pressure and *vice versâ*. The 12th and 13th dorsal nerves, for instance, cause usually a great renal contraction, but the accompanying rise of blood pressure is not so high s with some of the nerves above them. Hence we must conclude that in individual cases there may be small variations in the number of fibres going on the one hand to the kidney and on the other hand to the other abdominal viscera.

The contraction of the kidney occurs after a short latent period, and in a typical case it is sudden, marked, and very persistent, often lasting long after the excitation has ceased. The kidney then commences slowly to expand and along with this expansion the blood

pressure falls to its normal height. Generally the kidney does not quite regain its former volume, in other words, its vessels remain slightly contracted as a more or less permanent after-effect. This effect is so small that it is not accompanied by any appreciable rise of blood pressure. In some cases after the excitation has ceased, the blood pressure falls slowly but slightly below its previous height and then slowly regains its normal level. That is to say, the sudden and great rise of arterial tension is followed by slight, slow, and gradual fall. This fall of blood pressure is accompanied by a slight contraction of the kidney, the volume of the latter following exactly the fall and subsequent rise of blood pressure. This result is only occasionally seen when quick rates of excitation are used, but it becomes more frequent when such a rate as five per second is employed.

It has been seen with most of the nerves, but it is more common with the upper than with the lower dorsal. Its full significance will be alluded to later, but this result is no doubt due to the excitation of vaso-dilator fibres, the kidney effect being a passive one due to changes of blood pressure produced in other organs.

In a very small proportion of cases a rise of blood pressure is produced as usual, but the kidney effect is a mixed one, *i.e.*, there is first a slight expansion then a marked contraction. In a still smaller number of cases a renal expansion has been observed, generally accompanied by a slight rise of the general blood pressure, but occasionally no such rise has occurred. When the kidney expansion is accompanied by a rise of general arterial tension, it is no doubt due to the kidney vessels being passively dilated owing to active contraction having taken place elsewhere. When, however, the expansion of the kidney is unaccompanied by any rise of pressure, it is difficult to avoid the conclusion that it is due to the excitation of actual vaso-dilator fibres; however, better evidence than this will be adduced in support of the existence of these nerves.

II. *The Existence and Course of the Vaso-dilator Fibres.*

Hitherto no definite evidence has been adduced in support of the existence of vaso-dilators for the vessels of the kidney. If, however, the 11th, 12th, or 13th dorsal nerves be excited by slow rhythmical shocks, *i.e.*, one per second, it will be found that expansion of the kidney occurs unaccompanied by any rise of blood pressure. This renal expansion is marked in character and rather persistent in its duration, that is to say, the organ does not return completely to its original volume after the cessation of the excitation. It is clear that the renal expansion is an active one, since the nerve stimulation has produced no obvious effect on the blood pressure. This striking result is not so easily obtained with the higher nerves; with these the same excitation produces a fall of blood pressure, accompanied

not by any expansion, but by a passive contraction of the kidney vessels. In other words, with these higher nerves a dilatation is produced, not only of the kidney vessels, but also of the vessels of a much larger area, and hence the renal dilatation is unable to manifest itself.

This view is confirmed by the results obtained on excitation of the splanchnic nerve. When this nerve is stimulated with quick rates, the kidney, as is well known, undergoes great contraction, and there is at the same time a large rise in the general blood pressure. With slow rhythmical stimulation, however, I have never succeeded in getting any renal expansion. This slow stimulation, however, causes a large fall in the blood pressure, accompanied by a marked renal contraction. This renal contraction is obviously passive, since it not only exactly follows the fall of blood pressure, but, when the exciting current is shut off, the blood pressure undergoes a sudden and temporary rise, and this rise is accompanied by a correspondingly transitory renal expansion. In other words, the dilatation is one produced in a large area, and the kidney vessels are affected secondarily. Hence just as the renal constrictor fibres are best marked in the 11th, 12th, and 13th dorsal nerves, so the same is true for the dilator. These, however, like the constrictors, probably exist in the higher nerves, but for the reasons given it is almost impossible to demonstrate their existence positively, as they run with the dilator fibres for the vessels of the other abdominal viscera.

Excitation of the peripheral end of the divided vagus in the neck causes of course marked contraction of the kidney, owing to its inhibitory action on the heart, which action is not obviated by the doses of curare employed. After small doses of atropine the stimulation of the cervical vagus has no effect on the volume of the kidney. Stimulation of the vagus in the thorax, *i.e.*, beyond the point where it gives off its cardiac fibres, has also no effect on the volume of the kidney. Thus we may conclude that there is no evidence to show that the vagus supplies any fibres to the renal vessels.

III. *The Reflex Phenomena of the Renal Vessels.*

Excitation of the central end of the divided *sciatic* causes, as shown by Roy, a contraction of the kidney accompanied by a rise of blood pressure. This result I can confirm, as it occurs in by far the greater number of cases. Occasionally, however, this nerve causes a slight expansion of the kidney, but this is not only very small in amount, but it is also very rare. Sometimes, as is well known, the central end of the sciatic causes a fall of blood pressure, and when this occurs it is accompanied by a renal contraction. The central end of a divided *intercostal nerve* causes a slight rise of blood pressure, accompanied by a small contraction of the kidney vessels.

The central end of the divided *vagus* in the rabbit causes a contraction of the kidney, accompanied of course by a rise of blood pressure. In the dog this is also by far the most common result. In the cat, however, and occasionally in the dog, the excitation of this nerve causes a depressor effect, *i.e.*, a fall of blood pressure, and with this fall a passive shrinking of the kidney. The central end of the *depressor* in the rabbit or of the vagus in the cat causes, as just mentioned, a great fall of blood pressure, accompanied by a passive contraction of the kidney. Although the blood pressure fall is always a large one, the effect on the kidney volume is but slight. Here again this effect is probably simply the result of the great dilatation of the other abdominal vessels, neutralising, so to say, the renal dilatation, and so causing an actual diminution in the volume of the kidney. In a few cases in the rabbit, where the blood pressure fall has not been very great, an initial slight expansion of the kidney has been detected.

The stimulation of the central end of a divided *posterior root* produces in almost all cases a great rise of general blood pressure. This rise is not only large in amount, but it is very sudden, and also of rather short duration. The pressure remains at the maximum height but a few seconds, and when the excitation is over falls towards its normal height; there is, however, generally a persistent after-effect, that is to say, the pressure remains a little higher than it was previously to the stimulation. There is no very material difference between the results obtained with the lower dorsal nerves and those seen with the upper ones, in both cases a large rise of pressure is obtained; on the whole, however, the reflex rise seen with the lower nerves is somewhat greater than that obtained with the upper nerves. As a rule the rise of pressure is accompanied by a contraction of the kidney, marked in amount, but not of such a persistent character as that described above as following the excitation of the peripheral end of an anterior root. Frequently the kidney effect is a mixed one, *i.e.*, a contraction followed by an expansion; not uncommonly, however, there is an initial expansion, the subsequent course of which is interrupted by a contraction. More frequently still no contraction of the kidney is seen, it is replaced by a pure expansion, accompanied as before, however, by a great rise of blood pressure. This effect, however, is most often obtained with the lower dorsal nerves, *e.g.*, the 10th to the 13th. Sometimes when the stimulation of a posterior root gives the renal expansion and rise of blood pressure, the application of the electrodes to the posterior surface of the cord gives an equal rise of blood pressure, accompanied, however, by contraction of the kidney. Hence the former effect, *i.e.*, the renal expansion, is the result of a more local excitation. When the reflex excitation causes expansion of the kidney there is profuse hæmorrhage from the spinal wound. Now this hæmorrhage

is not altogether to be explained as resulting simply from the heightened blood pressure, since an equal rise, produced say by the sciatic and accompanied by contraction of the renal vessels, is not followed by this profuse hæmorrhage. Hence it is probable that not only is there a dilatation of the kidney vessels, but also of the vessels in the lumbar region of the body wall, hence the hæmorrhage.

Rarely excitation of a posterior root causes a depressor effect, there being a great fall of blood pressure, and then as usual the kidney undergoes a passive contraction, owing to the large dilatation elsewhere.

The results of reflex excitation can then be summed up shortly by saying that the excitation of an afferent nerve causing a rise of blood pressure is accompanied by a renal contraction, unless the nerve is one of what may be called the renal area. In this case the rise of blood pressure is accompanied as a rule by either a renal expansion or else by a mixed kidney effect. If the afferent nerve causes a depressor effect due to dilatation of the abdominal vessels, the kidney vessels probably share in that dilatation, but this is not seen by any actual renal expansion owing to this being overpowered by the dilatation elsewhere, and hence the kidney undergoes a passive shrinking.

The other conclusions of this paper are that the renal constrictor fibres leave the cord through the anterior roots of the nerves extending from the 6th dorsal to the 2nd lumbar inclusive.

That, secondly, there are vaso-dilator fibres, as can easily be demonstrated with such nerves as the 11th or 12th dorsal, but that in all probability they also extend from the 6th dorsal to the 2nd lumbar, and for the reasons given above they cannot be demonstrated with certainty in the upper nerves, since here they run with the vaso-dilator fibres for the vessels of the other abdominal viscera.

Hence there is no evidence to show that the vaso-constrictor fibres and the vaso-dilator fibres reach the kidney by different routes.

Finally, the great splanchnic nerve contains not only vaso-constrictor, but also vaso-dilator fibres, for the vessels of the abdominal viscera.

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III. "The Innervation of the Pulmonary Vessels." By J. ROSE BRADFORD, M.B., D.Sc., George Henry Lewes Student, and H. PERCY DEAN, M.B., B.S., B.Sc. Communicated by E. A. SCHÄFER, F.R.S. (from the Physiological Laboratory of University College, London). Received February 13, 1889.

Although hitherto most physiologists have considered that the pulmonary vessels probably possessed a system of vaso-motor nerves, yet no direct experimental proof of the existence of such a system has been obtained. Still less has any evidence been adduced to demonstrate the actual anatomical paths by which such nerves, if they exist, reach the lungs. Hence it seemed that the whole question was one deserving a further attempt for its solution. When this research was commenced, there were practically only two facts which could be appealed to in support of the existence of these nerves.

Firstly, Lichtheim observed that in asphyxia a rise of blood-pressure may occur in the pulmonary artery unaccompanied by any rise in the aorta.

Secondly, it has been shown that in the frog, irritation of the skin causes a contraction of the pulmonary vessels.

It is clear that this second fact could not be used as an argument in support of the existence of these nerves in the mammal, since the anatomical relations are so different in the two cases.

With regard to Lichtheim's observation, it is evident that it affords no very direct proof, since other conditions, such as venous distension, might easily account for the rise of pulmonary pressure.

It was felt by us that the only really reliable method would be to excite one by one the roots of the spinal nerves, and to observe the effects of such stimulation on the aortic and pulmonary blood-pressures simultaneously.

The following method was employed:—A cannula, placed in the carotid artery in the usual manner, was connected with a mercurial manometer. In a similar manner a second mercurial manometer was then connected with the branch of the left division of the pulmonary artery distributed to the lower lobe of the left lung. This vessel was reached from the back by resecting portions of two or sometimes three ribs. In this way a record of the pressure in the left division of the main artery was obtained, and also a means of detecting changes of pressure in the main artery. At the same time, the minimum amount of lung tissue was thrown out of gear.

The upper dorsal nerves were then exposed inside the spinal canal, and were ligatured outside the dura mater. By cutting through the

nerves between the spinal cord and the ligature, the peripheral ends could be easily arranged for excitation.

In this way the fibres of both anterior and posterior roots are excited, but, as previously shown by one of us, no efferent vaso-motor fibres can be demonstrated to exist in the posterior roots. Hence for our purposes this mode of excitation is practically equivalent to exciting the anterior roots alone, and inasmuch as a comparatively long stretch of tough nerve can be obtained, the danger of the exciting current spreading to the spinal cord, and so producing reflex effects, is avoided. The nerves were excited on the right side, on the same side as the uninjured lung.

The two blood-pressure curves were recorded simultaneously on the same blackened surface, together with a time tracing and a lever marking the duration of the excitation.

The anæsthetics used were chloroform and morphia, and after the nerves had been prepared, a small dose of curare was injected, and artificial respiration maintained before opening the chest to insert the pulmonary cannula.

Before describing the results following excitation of the upper dorsal nerve roots, it will be necessary to describe shortly the relations existing between the systemic and pulmonary blood-pressures, and more especially what effects are produced on the pressure in the pulmonary artery by sudden alterations of the blood-pressure in the systemic vessels. It is necessary to do this, as otherwise in many cases it might be urged that the effects of a given nerve excitation on the pulmonary pressure were simply due to the reaction of the pulmonary vessels to the accompanying carotid rise. In some cases this objection has no force whatever, since there is no carotid rise or there may even be a carotid fall. In other cases, *e.g.*, in stimulation of the fifth dorsal nerve, there is often a rise of blood-pressure in both vessels, and so we see how important it is to get a clear notion as to what effect a given rise of arterial tension has on the pulmonary blood-pressure.

Before describing the results we have obtained in this direction, it will be convenient to consider shortly the actual amount of the pulmonary blood-pressure, and the manner in which it is influenced by artificial inflation of the lungs.

The pressure is found to vary between 16 mm. and 20 mm. of mercury in different dogs, these being the animals we have always used in our experiments. The pressure in the main artery is a few millimetres higher than this.

The pulmonary pressure is very constant in its height, not only in the same animal during the course of an experiment, but also in different animals. In this point it contrasts strongly with the aortic pressure, since the latter is very variable in amount after the necessary

operative procedure described above. The aortic pressure must fall very low indeed for the pulmonary pressure to be appreciably diminished in amount. The following is an instance bearing out the truth of this statement.

Section of spinal cord at level of seventh dorsal nerve caused the aortic pressure to fall from 106 mm. *Hg* to 52 mm. *Hg*. The pulmonary pressure fell from 16 mm. to 14 mm. Hg. Thus, while the aortic pressure fell to half its previous height, the pulmonary pressure only diminished by one-eighth of its previous amount.

Artificial inflation of the lungs causes a rise of pressure in both systems followed by a fall during the subsequent expulsion of the injected air. The pulmonary rise is more sudden and marked in character than the aortic rise, but the rise and fall of pressure in the two vessels are, as far as can be determined, quite synchronous.

The effect of artificial inflations is the same, whether the vagi are intact or whether they have been previously divided.

We will now turn our attention to the effects produced on the pulmonary blood-pressure by a sudden increase in the aortic pressure. It is evident that this rise of pressure in the systemic circulation must be produced in such a way as to avoid stimulating, if possible, the vaso-motor centre reflexly, although, as we shall see later on, the results obtained by reflex excitation are also valuable in deciding this question.

Three methods have been used by us to produce a large rise of blood-pressure in the systemic circulation, and so to determine the passive effect of this rise on the pulmonary circulation. They are as follows:—

I. The excitation of the peripheral end of a divided splanchnic.

II. The excitation of the lower end of the spinal cord divided in the middle of the dorsal region, and care being taken that no spreading of the current to the central end occurs.

III. Compression of the thoracic aorta.

I. Results obtained by Excitation of the Peripheral End of a divided Splanchnic.

The rise of systemic blood-pressure is of course considerable, in many cases it is doubled. The rise of pressure in the pulmonary artery is not, however, very marked. Thus in one case an excitation lasting 48 seconds produced a rise of aortic pressure amounting to 54 mm. Hg. The accompanying rise of pulmonary pressure was only 3 mm. Hg.

The aortic pressure was rather more than doubled, having risen from 50 mm. Hg to 104 mm. Hg, on the other hand, the pulmonary rise was from 13 mm. Hg to 16 mm. Hg, the mean rise being, however, 2·5 mm. Hg.

These results are curiously similar to those mentioned above, where

a fall of aortic pressure from 105 mm. to 52 mm. Hg was accompanied by a pulmonary fall of only 2 mm. Hg.

Thus in two different animals sensibly the same effects were produced in the pulmonary pressure in opposite directions by practically equal changes of pressure in opposite directions produced in the aortic pressure.

II. *Results obtained by Excitation of the divided Spinal Cord.*

Excitation of the lower end of the divided cord produces an enormous rise of general blood-pressure, but the accompanying rise of pulmonary pressure is not only always small but it is frequently absent.

Thus in one case stimulation for 38 seconds caused a rise of general blood-pressure amounting to 180 mm. Hg, and the simultaneous pulmonary rise was 6 mm. Hg. This is an extreme case. In many instances the pulmonary rise was less than this, even when the aortic rise was quite as marked. In this case the aortic pressure rose from 52 mm. Hg to 232 mm. Hg, and the pulmonary pressure from 20 mm. Hg to 26 mm. Hg, thus although the aortic pressure was quadrupled, the pulmonary pressure was only raised by less than one-third of its previous amount.

III. *Results obtained by Compression of Thoracic Aorta.*

When this vessel is compressed about the middle of the dorsal region by the finger introduced through the wound, the aortic pressure measured in the carotid undergoes a great and sudden rise, followed on removing the finger by a transitory fall. If the compression be maintained for only a short time, *e.g.*, 10 seconds, then there is no rise of pulmonary pressure, although, of course, the aortic pressure will have been greatly augmented, in this case from 104 mm. to 169 mm. Hg, a rise of 65 mm. Hg.

If, however, the compression be maintained longer, then the pulmonary pressure rises as we see from the following experiment:—The aorta was compressed for 30 seconds, and the aortic pressure rose from 71 mm. to 128 mm. Hg, and that in the pulmonary artery from 19 mm. to 22 mm. Hg.

In all three of the preceding series of experiments the pulmonary rise is very small when compared with the enormous effects produced in the aortic pressure. In all these cases the pulmonary rise was roughly one-twentieth of the simultaneous rise in the systemic circulation. Not only is the rise of pulmonary pressure small when compared to the aortic rise, but the actual pulmonary rise is but a small fraction of the total pulmonary pressure. Thus, although some of the above methods may double or even quadruple the aortic pressure,

yet none of them causes anything like a doubling of the pulmonary pressure.

In other words, when a great aortic rise has succeeded in producing a pulmonary rise, the latter is not only small relatively to the aortic rise but also relatively to the pulmonary pressure itself. We may conclude that not only must a great rise of aortic pressure occur in order to produce any appreciable rise of pulmonary pressure, but also that this rise must be of some duration.

The further discussion of the mode in which a rise of aortic pressure produces a rise of pulmonary pressure will be entered into at the close of this communication. Having thus described shortly what may be called the mechanical effects of rises of aortic pressure on the pulmonary circulation, we will now consider the results of reflex excitation of such nerves as the *sciatic* and *vagus*.

Results of Excitation of the Central End of the divided Sciatic.

It is well known that the rises of aortic pressure produced by the excitation of this and other afferent nerves are frequently very considerable. This is especially the case with the sciatic nerve.

In one case the stimulation of the central end of this nerve gave an aortic rise of 36 mm. Hg, and the accompanying pulmonary rise was only 2 mm. Hg, *i.e.*, one-eighteenth of the aortic rise, that is to say, nearly the same ratio as that obtained in the previous experiments described above in the passive reaction of the pulmonary vessels to rises of general arterial tension. In another instance, with an aortic rise of 30 mm. Hg, there was no simultaneous pulmonary rise.

Results following Excitation of the Central End of divided Vagus.

With this nerve somewhat different results are obtained.

Thus, in one case, the aortic rise was 32 mm. Hg and the pulmonary rise 4 mm. Hg, *i.e.*, the relative ratio of the two effects being one-eighth. This result was obtained in the same animal that previously gave with the sciatic a ratio of one-eighteenth. In the case of the vagus the pulmonary rise was double that observed with the sciatic, although the aortic rises were almost the same in the two cases, *i.e.*, 36 mm. and 32 mm. Hg. It is clear then that, although in this animal the vagus and sciatic gave on stimulation practically equal effects in the systemic vessels, yet the results on the pulmonary vessels were by no means the same in the two cases. Hence the only conclusion is that excitation of the central end of the divided vagus caused a reflex contraction of the pulmonary vessels and thus caused a heightened pulmonary tension.

In the cat frequently and in the dog occasionally the stimulation of the central end of the vagus causes a fall of blood-pressure instead of a rise, in many cases the fall of aortic pressure is considerable. Thus

in one experiment the central end of left vagus was excited for 28 seconds and the aortic pressure fell from 112 mm. to 66 mm. Hg, *i.e.*, a fall of 56 mm. Hg. The pulmonary pressure fell from 17 mm. to 14 mm. Hg, *i.e.*, a fall of 3 mm. Hg.

This pulmonary fall is rather greater in amount than that previously described as occurring after section of the cord in the dorsal region, but it is not too large to be explained on the grounds of a passive effect owing to the large aortic fall.

It is, however, with stimulation of the posterior surface of the spinal cord that the greatest relative effects are seen. When this mode of excitation is used the rise of pulmonary pressure is frequently as much as one-tenth of the simultaneous aortic rise, *i.e.*, the ratio is higher than with any of the previous methods of experimentation.

No doubt part of this effect may be due to the direct excitation of the pulmonary vaso-motor fibres, as will be shown below. Probably, however, the result is mostly due to reflex effects dependent on the cord stimulation, and this is confirmed by the fact that excitation of the central end of a divided posterior root of the upper nerves will cause a great relative rise of pulmonary pressure.

On the other hand, the excitation of the central end of a divided intercostal nerve causes but slight effects both on the pulmonary and on the aortic blood-pressures. Occasionally the central end of an intercostal produces depressive effects similar to those just described for the vagus.

Having thus determined the relation existing between a given rise of aortic pressure and the coincident passive pulmonary rise, and also the effects resulting from reflex excitation of the cord, vagus and sciatic, we will now pass on to the question of the existence and paths of the vaso-motor fibres.

If the upper part of the medulla oblongata be excited it will, of course, be found that a large rise of aortic and pulmonary pressure will be observed. If, now, the spinal cord be divided at about the level of the 7th dorsal nerve and its lower end excited, then just as great or perhaps greater rise of aortic pressure will be observed, but the pulmonary rise will be either very small indeed or else entirely absent.

If the upper part of the medulla be now again excited, the rise of aortic pressure is small owing to the section of the cord, but the pulmonary rise is as great as before. With stronger excitation this rise of pulmonary pressure becomes greater whilst the accompanying aortic rise is still comparatively small. Thus, in one case, the excitation of the lower end of the divided cord caused an aortic rise of 150 mm. Hg. The accompanying pulmonary rise was less than 2 mm. Hg. On now exciting the medulla in the same animal the aortic pressure rose 55 mm. only, owing to section of the cord, but the pulmonary pressure

rose from 16 mm. to 22 mm. Hg, *i.e.*, 6 mm. Thus in the latter case the aortic rise was one-third of what it was in the previous experiment, but the pulmonary rise was three times as great.

This experiment then clearly demonstrates that the pulmonary pressure is not dependent on the aortic rise, since the latter can be obtained without the former, and a pulmonary rise, very considerable in amount, can be obtained when the aortic rise is either small or large.

Hence this result points strongly to the conclusion that the vaso-motor centre can influence the pulmonary vessels directly. In the light of Gaskell's work on the sympathetic, we naturally turn to the roots of the upper dorsal nerves, and we are enabled to map out the paths by which these vaso-motor nerves reach the lung.

When the peripheral end of such a nerve as the 6th or 7th dorsal is excited a rise of pressure in both the pulmonary and aortic system is observed. The pulmonary rise, although considerable, *e.g*, 3 or 4 mm. Hg, is not out of proportion to the aortic rise which, with these nerves, may be as much as 30 or 40 mm. Hg. On ascending, however, very different results are obtained. Thus in one case the 5th dorsal gave an aortic rise of 10 mm. Hg only, but the pulmonary rise was 3 mm. Hg. Clearly the latter was not a passive effect of the former. In another case the 4th dorsal gave an aortic rise of 20 mm. Hg, and a pulmonary rise of 4 mm. Hg.

Perhaps, however, the most marked and conclusive result is seen with the 3rd dorsal nerve. This nerve frequently causes no aortic rise, and, indeed, sometimes actually a fall, *e.g.*, 10 mm. Hg, but in both these cases there is a distinct pulmonary rise of 3 or 4 mm. Hg. We sometimes get such a fall in the aortic pressure accompanied by a pulmonary rise with the 4th nerve and twice we have seen it with the 5th nerve.

As a rule these effects cannot be obtained when the accelerators produce marked effects, and hence no very definite results have been obtained from stimulation of the 2nd dorsal nerve. Often, however, the heart is already beating rapidly, so that irritation of the accelerator nerves causes no further increase in rate, and it is under these circumstances that the pulmonary vaso-motor fibres can be most easily demonstrated. Thus, as we pass from the 7th to the 2nd nerve, the effect of their excitation on the aortic pressure diminishes as we pass from below upwards, and the upper nerves may even cause a fall of pressure in systemic circulation. On the other hand, the effect on the pulmonary pressure seems to increase as we pass from below upwards. Hence we may conclude that the vaso-constrictor fibres for the lungs leave the spinal cord in the roots of the dorsal nerves from the 2nd to the 7th.

An attempt was made to separate the pulmonary nerves from the

cardiac nerves in the branches of the ganglion stellatum and in the annulus Vieussenii. As yet, however, we have not been able to separate the pulmonary vaso-motor fibres from the accelerator fibres.

The objection will of course be made that the effects are slight, and no doubt they are, but when we consider that enormous changes in the aortic pressure produce such extremely slight effects, it is clear that, small as those effects are, they conclusively show that they are dependent on the contraction of the pulmonary vessels, and not on any passive effect from the slight rises in the aortic pressure.

There seems no doubt that the vaso-constrictor mechanism of the lungs is not very highly developed. It is impossible to get anything like a doubling of pulmonary blood-pressure by any kind of nerve excitation, although the systemic blood-pressure can easily be doubled or even quadrupled. The amount of possible contraction of the pulmonary arterioles is probably not nearly so great as that of the systemic vessels, and this view is confirmed by the results of asphyxia on the pulmonary circulation.

Results of Asphyxia on the Pulmonary Circulation.

In asphyxia both the aortic and the pulmonary blood-pressures undergo a considerable rise, but the rise of pressure in the pulmonary vessels lasts longer than that in the systemic, so that when the aortic pressure is falling rapidly, the pulmonary may be at its highest point.

The rise of pressure occurs synchronously in the two sets of vessels, and the general course of the two curves is the same, except that the pulmonary rise is more gradual than the aortic rise. As a rule, the sudden and great elevations seen on the aortic blood-pressure curve are not well seen on the pulmonary trace, but notwithstanding this, the maximum rise of the pulmonary pressure may be very considerable, *e.g.*, it may be doubled.

If, however, so large an effect as this is seen, the aortic pressure will have undergone a very much greater relative rise, *i.e.*, it will have been quadrupled.

The Traube curves, so well marked on the aortic blood-pressure tracing, are but faintly marked in the case of the pulmonary artery, and hence it is difficult to say whether the effects are direct or due simply to passive reaction from the systemic circulation. It is probable, however, that they are direct.

The curious maintenance of the pulmonary pressure at such a height as death approaches, when the aortic pressure has fallen perhaps to half its previous height, is probably due to venous distension as much as to the increased peripheral resistance, but this is a point we wish to investigate further.

Conclusions.

The pulmonary vessels of the dog are supplied with vaso-motor fibres leading the cord through the roots of the uppermost dorsal nerves. No efferent vaso-motor fibres have been detected in the vagus nerve.

The pulmonary circulation is comparatively independent of the systemic, and alterations in the blood-pressure of the latter must be of large amount to affect the pulmonary blood-pressure. It is probable that no rise of aortic pressure can materially influence the pulmonary blood-pressure, unless it is so great in amount or duration that the heart muscle and valves are unable to cope with it, and so an actual regurgitation is produced.

It is possible that the pulmonary blood-pressure can also be affected by rises of systemic pressure causing venous distension, and hence an increased supply to the right side of the heart.

Finally, although it is undoubted from the results of this research that the mammalian pulmonary vessels receive vaso-motor nerves, yet it is probable that the vaso-motor mechanism is but poorly developed as compared with that regulating the systemic arteries.

In this respect it may be that the pulmonary system holds an intermediate position between the systemic arteries on the one hand and the veins on the other.

This question we hope to elucidate by a further research. We also hope that, shortly, we shall be able to give the results of our researches on the vaso-dilator nerves of the lungs.

Presents, February 21, 1889.

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February 28, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Spectra of Meteor-swarms (Group III)." By J. NORMAN LOCKYER, F.R.S. Received February 14, 1889.

I. *Introductory.*

Up to the present time the prevailing idea has been that nebulæ, stars, and comets represent different orders of bodies in the cosmos, and all classifications have proceeded on the assumption not only that these bodies are variously constituted but that in the case of the "stars" all are becoming cooler. In a paper communicated to the Royal Society in 1865,* Dr. Huggins writes: "My observations, as far as they extend at present, seem to be in favour of the opinion that the nebulæ which give a gaseous spectrum are systems possessing a structure and a purpose in relation to the universe, altogether dis-

* 'Roy. Soc. Proc.,' vol. 14, p. 39.

tinct and of another order from the great group of cosmical bodies to which our sun and the fixed stars belong."

With regard to the most generally accepted classification of stars, that of Vogel, Dunér ("Étoiles à Spectres de la 3me Classe") writes, "Selon la théorie il faudra que tôt ou tard toutes les étoiles de la première classe deviennent de la seconde, et celles-ci de la troisième."

Vogel, and before him, others, working on the assumption that all the heavenly bodies were reducing their temperature, practically included all stars between the hottest and the coldest in one class (Class II*a* of Vogel).

In previous papers to the Royal Society I have adduced evidence to show that all cosmical bodies are or have been meteor-swarms, that at the present time some are increasing and some are reducing their temperature. Thus, in the Bakerian Lecture, 1888, I demonstrated that nebulæ and stars of Group II (Class III*a*) are still increasing in temperature by the condensation due to gravity, and that the red stars of Group VI (Class III*b*) are at a nearly equal mean temperature to stars of Group II, but are cooling bodies.

In these extreme cases the differentiation between the two groups was comparatively easy. In the case of those stars which are a little less hot than the hottest, whether they are getting hotter or cooler, the spectral difference cannot nearly be so well marked, as both classes will have line spectra; but it was essential to my hypothesis that these bodies should be resolvable into two groups, one increasing and one decreasing in temperature, with spectra proper to each.

The object of the present paper is to set forth the evidence which shows that this differentiation is possible, and to suggest the lines along which future researches on the subject might follow.

In this paper, which is only to be regarded as preliminary, I purpose to state the information already obtained with respect to Group III, and its relation to the two groups which bound it, in order that the validity of the distinction that I have drawn may be further tested. At present the observations are not sufficiently detailed to enable a classification into species to be made, as was done for Group II, so that we have to be contented with a general statement of the sequence of phenomena in passing from the early to the later stages of the group.

The observations lay no claim to great accuracy; only small dispersion has been employed, and only a reconnaissance has been attempted. The general method has been first to observe the differences between stars like Capella, which mostly resemble the sun, and those like α Cygni and α Tauri, which show marked variations. In this way the criteria which are hereafter enlarged upon were determined.

Particular attention was directed to the manner in which the flutings which form the special characteristic of Group II died out in

passing from Group II to Group III; and what other phenomena accompanied the transition, and what were the special phenomena which accompanied the gradual distension of the hydrogen lines in passing to Group IV. There has not been a sufficient number of fine nights since the work commenced to enable this to be done completely.

II. *General Statement of Conditions.*

A general statement of the conditions of the problem was given in the Bakerian Lecture (p. 26); and I here reproduce the greater part of what I then wrote on the subject.

"The passage from the second group to the third brings us to those bodies which are increasing their temperature, in which carbon radiation and fluting absorption have given place to line absorption. At present the data already accumulated by other observers have not been discussed in such a way as to enable us to state very definitely the exact retreat of the absorption—by which I mean the exact order in which the absorption lines fade out from the first members to the last in the group. We know generally that the earlier bodies will contain the line absorption of those substances of which we get a paramount fluting absorption in the prior group. We also know generally that the absorption of hydrogen will increase while the other diminishes.

"The next group—the Fourth—brings us to the stage of highest temperature, to stars like α Lyræ, and the division between this group and the prior one must be more or less arbitrary, and cannot at present be defined. One thing, however, is quite clear, that no celestial body without all the ultra-violet lines of hydrogen discovered by Dr. Huggins can claim to belong to it.

"We have now arrived at the culminating point of temperature, and next pass to the descending arm of the curve. The Fifth Group, therefore, will contain those bodies in which the hydrogen lines begin to decrease in intensity, and other absorptions to take place in consequence of reduction of temperature.

"It seems fair to assume that physical and chemical combinations will now have an opportunity of taking place, thereby changing the constituents of the atmosphere; that at first, with every decrease of temperature and increase in the absorption, lines may be expected, but it will be unlikely that the coolest bodies in this group will resemble the coolest bodies in Group III.

"Up to the present time observers have not recognised the importance of these considerations, and since only one line of temperature, and that a descending one, has been considered, no efforts have been made to establish the necessary criteria between Groups III and V."

It follows from the above that criteria are only possible from the

fact that on the ascending side of the curve the varying volatilities of the meteoritic constituents of the swarms brought out by successively higher temperatures are in question, whilst on the descending side of the curve we have to deal with successive chemical combinations, brought about by a fall of temperature in a gaseous mass.

III. *Relation between the Early Species of Group III and the Later Species of Group II.*

Since bodies of Group III are produced by the further condensation of the condensing swarms which I have included in Group II, there must be a close relation between the earlier species of Group III and the later species of Group II; that is, if there be anything like the continuity which my hypothesis demands. We know, for instance, that in the later species of Group II, there are flutings both dark and bright, and dark lines, amongst the latter being *b*, D, and E. As the lines are produced, so to speak, at the expense of the flutings, we should expect to find that lines of magnesium, sodium, manganese, and iron are the most prominent, especially in the earlier species of Group III. In α Orionis we have associated with the metallic flutings the lines *b* and D, and both are well developed, E is also present, but it is not nearly so strong as *b* or D. The F line of hydrogen is shown as a thin line in a photograph of the spectrum taken by Professor Pickering, although, as far as I know, it had not been previously recorded. With an increase in temperature, a condensing swarm like α Orionis would give a spectrum without flutings; the magnesium flutings would be replaced by *b*, and the iron fluting would be replaced by iron lines, of which E and the line at 579 would be the most prominent. F is absent in most of the stars of Group II, because the radiation of hydrogen from the interspaces is just sufficient to balance the absorption; but in bodies of Group III, the interspacial radiation will have almost disappeared, and absorption will be predominant. We shall thus have F appearing thin in the early stages of Group III, and gradually thickening until it becomes as thick as in α Lyræ.

In the earliest stages of Group III we should therefore expect to find F and E thin and *b* and D thick. As yet we have no evidence as to the first appearances of dark *b* and D in Group II, but future observations made with special reference to this point will at once indicate in what species they first make their appearance as absorption lines.

With the next increase of temperature F and E will thicken, but *b* and D will show no marked difference. With a further increase *b* and D will lose their supremacy, and will be only of about the same thickness as F and E, because most of the magnesium and sodium would have been driven out with the first rise in temperature. Afterwards all the lines, except those of hydrogen, will gradually thin out on

account of the increased temperature. Finally, the spectrum will be of the type represented by α Lyræ.

The question here arises, where are we to draw the line between Group II and Group III? If my definition of Group II as the "mixed fluting" group be accepted, we must obviously draw the line at the stage where carbon radiation disappears. The iron fluting at 615 remains for a considerable time after this happens, so that the earliest species of Group III will be marked by the absorption fluting of iron in addition to the characteristic line absorption. This being the case, observations show that Aldebaran is a good example of an early stage.

IV. *The Relations of the Later Species of Group III to Stars of Group IV.*

The spectrum characteristic of Group IV is that of excessive hydrogen absorption, with other lines exceedingly faint. In passing from Group III to Group IV, therefore, the hydrogen lines must thicken whilst the metallic lines thin. In a letter to M. Dumas in 1872 I suggested that possibly the simplification of the spectrum of a star might be associated with the highest temperature of the vapour, and that idea seems to have been accepted by other investigators since that time. It is now generally accepted that stars with thick hydrogen lines (Group IV) are the hottest stars.

The reason why we have hydrogen absorption in such great excess, is, I have little doubt, that most other substances have been dissociated by the intense heat resulting from the condensation of the meteoric swarm. We are, in fact, driven to this conclusion, because the hydrogen which was originally occluded by the meteorites must have been driven off long before this temperature was reached.

In passing from a star like α Tauri to one like α Lyræ, the metallic lines would thin and disappear in some order determined by their dissociability or some other quality. The later stars of Group III are therefore very closely related to stars of Group IV, and the division between the two must be more or less arbitrary. For simplicity's sake, I have taken Group IV as the point of maximum temperature.

V. *The Observations having reference to Specific Differences in Group III.*

The observations have been made at the Astronomical Laboratory at South Kensington by Mr. Fowler, assisted by Messrs. Baxandall and Coppen (with the 10-inch equatorial and star spectroscope by Hilger) in connexion with my own observations at Westgate (made with a 12-inch mirror, kindly lent to me by Mr. Common, and a small Maclean spectroscopic eyepiece). All measurements and comparisons suggested by my own observations were made by my assistants, as at

present I have no means of doing this myself. The stars selected for observation were a few of the brightest hitherto known as belonging to Class IIa of Vogel's classification. A few stars more advanced than the IIa stars and a few less advanced were also observed in order that the passage from one group to the other might be determined.

The main points to which attention was directed were (1) the relative intensities of F, *b*, E, D, both in the same star and from star to star; (2) the lines which appear to be special to one group or the other (III or V).

The importance of observing the thickness of F in the spectrum of a star, as compared with its thickness in other stars, is obvious, for it at once enables us to fix the position of the star on the temperature curve immediately we have determined whether its temperature is increasing or decreasing.

Details of the observations of the thirteen stars which appear to be on the ascending side of the temperature curve are given below. One of these is a Group IV star, and one is a swarm of the last species of Group II. The remainder belong to Group III.

The stars are arranged in order of temperature, beginning with the lowest, as far as the observations enable us to do this. In general, the observations have been limited to the region of the spectrum lying between F and the iron fluting in the red at wave-length 615.

The wave-lengths of the lines and flutings were determined by direct comparison with the electric spark, and with the lines and flutings seen when the various substances are volatilised in the Bunsen burner. On one or two occasions, comparisons were also made with the spectrum of the Moon.

α Ceti.—F is fairly well seen, but it is not nearly so thick as *b* or D, and not quite as thick as E. D is pretty thick and lies in the Mn (2) fluting (586). *b* is also thick. The trio of lines* in the green is present, the most refrangible member being the darkest. Lines are present at about 579 and 568·5, the former being the stronger. Lines at 499 and 552 rather thin. The absorption Fe (1) fluting at 615 and Mn (2) are both present, but far less intense than in Mira Ceti. The flutings Mn (1) 558 and Pb (1) 546, are also both feebly visible. The brightest fluting of carbon at 517 is just perceptible. This is therefore a very late star of Group II. It is, in fact, the most advanced Group II star of which observations have at present been made.

ι Aurigæ.—Spectrum greatly resembles that of Aldebaran. F is thin. D is very thick, and more prominent than *b*. The trio of lines in the green is well seen. 579, 568, and the lines near 546·5 and 558 are well seen. The lines at 499 and 552 are also present. The iron

* The trio referred to in the observations comprises the lines E (5268), 5327, 540.

fluting at 615 is present, and is a little stronger than in Aldebaran. This and the relative thickness of F lead to the conclusion that the star falls between α Ceti and Aldebaran. Carbon 517 has disappeared.

α Tauri.—F, E, and 499 are all about the same intensity, but none of them are so strong as *b* or D. The trio is present, E being a little thicker than the second and third members. All three are seen to be double when a high power eyepiece is employed. 579 is nearly as thick as E and is stronger than 568. Groups of lines near 546 and 558 are fairly strong. 552 is also well seen. The Fe (1) fluting in the red (615) appears rather weak, and a pretty strong line runs through it near the most refrangible edge. There is also a line between *b* and 499, another between E and 5327, and many others. The Mn (2) (586) fluting is possibly visible at times.

χ Ophiuchi.—F is slightly stronger than in α Tauri, but is a little thinner than E. Not so thick as in α Cygni or α Serpentis. D is the strongest line in the spectrum and *b* comes next. 579 and 568 are both about the same intensity as E; so is 540, whilst the remaining member of the trio is rather weaker. The lines near 546 and 558 are fairly strong, as is also 499. The iron band in the red is absent.

β Ophiuchi.—F and E are about equally thick in the spectrum of this star. F is thicker than in Arcturus, but is not so thick as in α Cygni. The trio is complete, all the three lines being very well seen. *b* is the strongest line in the spectrum and D is the next. 568 is present, but weaker than 579. The lines near 546 and 558 are also certainly present. The line at 499 is about as thick as E. 552 is present.

ε Pegasi.—F is about as strong as in α Aquarii; D and *b* are about equal and as strong as F. 579 is stronger than 568 but is not quite so strong as D. E and 540 are not nearly as strong as 579; the other member of the trio (5327) is very distinct. 499 and the lines near 546 and 568 are all fairly strong. 552 is also present.

α Aquarii.—F and *b* seem about equal in intensity in the spectrum of this star. D is not quite so strong as *b*, but is a little stronger than E; the other two lines of the trio are rather faint. 579 is about as strong, or perhaps stronger, than E. 568 is much weaker than 579. 499 is a fairly strong line. The lines near 546 and 558 are well visible.

γ Aquilæ.—F is not nearly so strong as in α Aquilæ, and is a little thinner than in α Cygni. *b* and D are well defined, and about equal in intensity, whilst E is a little weaker; the remaining members of the trio are fainter than E. 579 and 568 are present, the former being about as strong as E, but the latter is barely visible. 499 and the faint lines near 546 and 558 are present; also an important line less refrangible than D, which was found by comparison with the electric spark to be near 598. Another fainter line was seen near 612.

α *Cygni.*—All lines except those of hydrogen are rather faint. F is the thickest line, G could not be seen very well, but C was well visible. D is fairly strong in comparison with *b* or E. E seems a little fainter than *b*, but stronger than the other members of the trio. 579 and 568 are seen, the former being much the stronger; it is almost as strong as D. The line near 499 is not very strong, and there appears to be a line on each side of it. The faint lines near 546 and 558 are also visible.

γ *Cygni.*—The lines are much easier to see and much more numerous than in α Cygni, although the whole spectrum is very much fainter. F is thicker than in α Cygni, and G is also visible. D and *b* are about equal in intensity, E is about the same as D, but much stronger than the other members of the trio. 579 is nearly as strong as E, but much stronger than 568. 499 is faint but certainly present. The lines near 546 and 558 are also present.

δ *Cygni.*—All the lines except those of hydrogen are faint. F and G are thicker than in γ Cygni, and therefore thicker than *b* or E, whilst E is thicker than the other members of the trio. 579 is a little stronger than 568. 499 and the lines near 546 and 558 are about equal, but very faint.

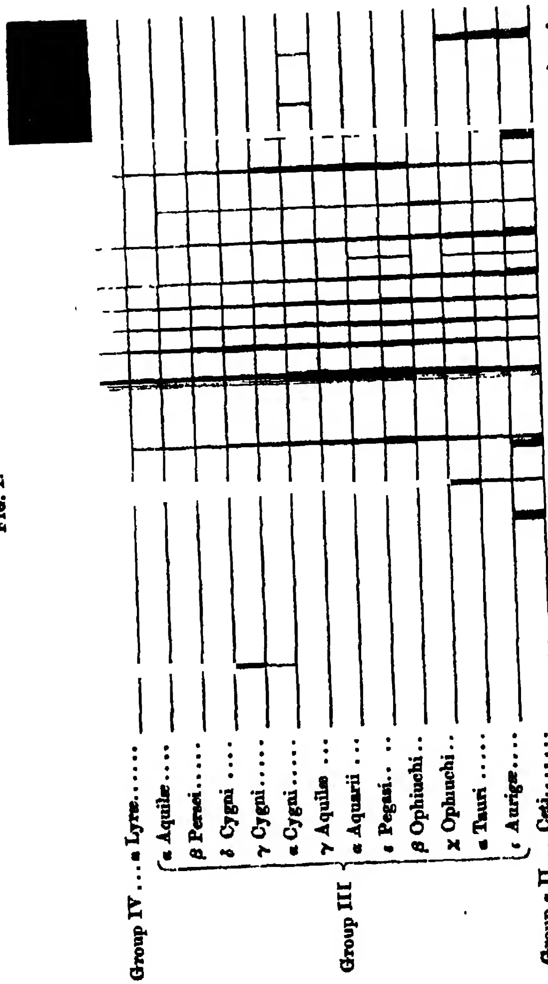
β *Persei.*—All lines faint with the exception of those of hydrogen. F and G both thick. *b*, D, and E are about equal in intensity. The remaining two members of the trio are also as thick as E. 579 is present, but 568 could not be seen. There is also a line near G, about 450; it is seen in the Henry Draper Memorial photograph of the spectrum of this star as a double, but it could not be resolved with the power used.

α *Aquilæ.*—All lines very faint except those of hydrogen. F and G very thick. *b*, D, and E very faint but about equal in intensity. 579 is not quite so strong as D. 499 is fairly well seen, as are also the two lines near 546 and 558.

α *Lyræ.*—All the lines except those of hydrogen are exceedingly faint. F is very strong but G is not quite so thick. *b* and D are fine lines, and about equal in intensity. The trio is undoubtedly present as also the lines near 546, 558, and 579.

The results of the observations which have been referred to are embodied in fig. 1. The star at the lowest temperature is on the lowest horizon, and the one at the highest temperature is on the top horizon. The thicknesses of the lines have been greatly exaggerated in the diagram, in order to render the variations more obvious.

Fig. 1.



Map showing the spectra of some of the stars of Group III, and of α Ceti and α Lyræ, arranged in order of temperature.

The wave-lengths and origins of the lines and flutings recorded in the observations are shown in the following table:—

Wave-length.	Origin.	Wave-length.	Origin.
434 (G)	Hydrogen.	552	Magnesium.
486 (F)	"	558 (fluting)	Manganese (1)
499	?	" (line)	?
5166 } (b)		568	Sodium.
5172 } (b)	Magnesium.	579	Iron.
5183 } (b)	Manganese.	586 (fluting)	Manganese (2).
5268 (E)	Iron.	589 (D)	Sodium.
5327	"	598	?
5400	Manganese.	612	?
546 (fluting)	Lead (1)	615 (fluting)	Iron.
" (line)	?		

VI. *Criteria between Groups III and V as deduced from the Observations.*

The general conclusion to be drawn from the observations is that there are several lines in the spectra of stars on the ascending side of the temperature curve, which do not occur in stars with a spectrum resembling that of the Sun, which must lie on the descending side of the curve, as we know it to be cooling.

Some lines, such as F, *b*, D, and E, are common to both sides of the curve, though the relative intensities are slightly different.

The principal criterion in the visible part of the spectrum is the double line about wave-length 540, which, with the two iron lines E (5268) and 5327, forms the trio referred to in the observations. Each member of the trio is seen to be double when a high power is used. These three equidistant lines, which are of nearly equal intensities, are well seen in Aldebaran and several other stars, but are not seen as such in either Arcturus or Capella.

In Arcturus and Capella, as in the Sun, there is a double line (5403, 5404·9) which makes an almost equidistant trio when combined with E and 5327. Direct comparison with Group III stars, however, shows that the lines are not coincident. On one or two occasions the spectra of some stars of Group III were compared with the spectrum of the Moon; in the absence of the Moon, comparison was made with Arcturus or Capella. A comparison of the Group III line with the Mn line at 540 referred to in previous papers shows a perfect coincidence with the dispersion employed; and since both are double we are driven to the conclusion that the 540 line in stars of Group III is due to manganese. Again, the double in Group V is considerably weaker than E, whereas that in Group III

is very nearly as strong as E. The appearance presented to the eye by the real trio in stars of Group III is accordingly very different from that presented by the three lines in stars of Group V.

Besides the least refrangible member of the trio there are other lines which are special to Group III. One of these lies between F and *b*, at wave-length 499, as nearly as can be determined with small dispersion. In some of the stars this line is very strong. It is only seen as a very faint line in Capella, Arcturus, or the Sun, and is consequently an important criterion. The nearest line of anything like equal importance in Group V stars is the iron line at 495·7.

Two lines, at 579 and 568 respectively, also appear to be special to Group III. No lines of similar intensities are seen in either Capella, Arcturus, or the Sun in those positions, although fainter lines are seen.

In Rowland's photographic map of the solar spectrum there is a line at 5659 which is much stronger than the one nearest to 568, and this is not seen at all in Group III stars. Only a very faint line is indicated in the same map at 5791, there being a stronger line at 5763 which is not seen in Group III stars. The two lines at 568 and 579 are, therefore, special to Group III. The line at 579 was compared directly with the low temperature iron line at 579, and the coincidence established with the dispersion employed; this may, therefore, be taken as due to iron. It may also be suggested that the line at 568 is the double green line of sodium, which appears bright in some of the bodies of Group I. Other lines referred to in the observations are near 546 and 558, but it is not easy to distinguish these from lines seen in stars of Group V. There are several strong lines seen in the solar spectrum in the neighbourhood of 546, and there are also strong lines at 5573 and 5587. In order to determine whether these lines will serve as criteria or not, further inquiry with greater dispersion will be necessary.

The magnesium line 5527 appears to be common to both Groups III and V, just as *b* is common to both.

There seems to be no doubt, therefore, that criteria between Groups III and V have been determined by the observations, and we are now in a position to assign the stars of Vogel's Class II*a* to one group or the other according as the lines which have been shown to be special to Group III are present or absent.

One of the chief objects I have had in view in writing this paper is to enable others to take up this important piece of work as soon as possible when once the idea of increasing and decreasing temperatures is generally accepted.

VII. *Tests.*

We have an important test of the accuracy of the preceding observations in tracing the continuity of the lines in passing from the earlier to the later species of the group. In the map which accompanies this paper, the stars have been arranged in order of temperatures by reference to the thickness of F, it being universally agreed that those stars in which the hydrogen lines are thickest are the hottest. With the stars in this order we ought to find that if a line be visible in any two of the stars, it is also visible in any other star of the group in which F is of an intermediate thickness. On first arranging the stars in this way, it was found that there were here and there breaks in the continuity of the lines, but further observations, made with special reference to the breaks, showed that the discontinuity was due to the incompleteness of the first sets of observations. The only break now shown on the map is the apparent absence of Mg 5527 in χ Ophiuchi, and this was not discovered before the star had got too far to the west to be re-observed.

We have another test in tracing the variations in the intensities of the various lines in passing through the series. Assuming that a sufficient number of stars have been taken, there ought to be no abrupt change in the thickness of a line in passing from star to star. The temperature at which a line is at its maximum thickness will depend on the volatility of the substance which produces it, so that all the lines need not necessarily have their greatest thicknesses in the same star. The continuity as regards the intensities of the lines is quite as perfect as could be expected from a preliminary survey. Thus D gradually thins from α Ceti to α *Lyræ*; *b* thickens from α Ceti to ϵ Pegasi, and then thins gradually to α Lyræ. This difference in the behaviour of *b* and D is obviously due to the fact that all the sodium would be distilled out of the meteorites before all the magnesium was driven out. E (5268), 5327, 540, and 499 gradually thicken to β Ophiuchi and then thin out. The line at 579 is almost equally thick in β Ophiuchi, ϵ Pegasi, α Aquarii, and γ Aquilæ. The line at 568 has a decided maximum in χ Ophiuchi. The lines near 546 and 558 have their greatest thickness in the earliest stage of the group, gradually thinning out towards the last. The remnant of the iron fluting (615) is seen to gradually disappear between α Ceti and α Tauri; no trace of this fluting was seen with the dispersion employed in any of the stars of a higher temperature than α Tauri. As the fluting disappears it is replaced by iron lines of gradually increasing intensities. The hydrogen line at G was not seen in any of the stars below α Cygni, but it does not follow that it was absent, because the lower stars being generally fainter, the attention of the observers was not directed so far into the blue.

It will be seen, then, that the continuity is practically perfect, both as regards the intensities of the lines and the presence in each star of the lines necessary for perfect continuity.

VIII. *Sequence of Spectra in Group III.*

The general sequence of spectra in passing from the earlier to the later species of Group III is as follows, as far as the observations have at present gone:—

(1.) The hydrogen lines are thin. D is thicker than *b*. The iron fluting is faint. 499, E, 5327, 540, 568, and 579 are thin. 546 and 558 are fairly thick.

(2.) The hydrogen lines are thicker. F, D, and *b* are equally thick. E, 5327, 540, 579, and 499 are much thicker, being nearly as strong as F. The iron fluting has gone.

(3.) The hydrogen lines are very much thicker than the other lines. D and *b* are equally thick. E is nearly as strong as *b*, while the other lines are fainter.

(4.) The hydrogen lines are very broad, while all the remaining lines are exceedingly faint.

Subsequent work will no doubt enable us to further divide these sub-groups into finer species.

II. "On the Magnetic Action of Displacement-currents in a Dielectric." By SILVANUS P. THOMPSON, D.Sc., B.A. Communicated by Professor G. CAREY FOSTER, F.R.S. Received February 19, 1889.

(Abstract.)

According to Maxwell's well-known views of electrostatic action, the variations of electric displacement which occur during the charge or discharge of a dielectric are to be regarded as equivalent to electric currents. No direct experimental proof of this point has hitherto been forthcoming. The author having calculated out on the assumption of the equivalence between displacement-currents and conduction-currents, what the effect would be of the charge or discharge of a condenser upon a delicately astatised needle placed near the edge of the condenser, concludes that the effects would be too delicate to be measurable. He therefore resorted to a different method based upon the principle that, if a closed curve be drawn around the flux of electrostatic displacement, the line-integral of the magnetising force, reckoned once round this closed curve, will at any instant be a measure of the rate of change in the electric displacement through the curve. Two forms of apparatus for realising this in an experi-

mental way were constructed. In the more satisfactory form of the apparatus an iron annulus surrounded by a coil of fine silk-covered copper wire is embedded in a layer of paraffin wax between two glass plates, and pieces of tinfoil are affixed on the outside surfaces of the plates to serve as the coatings of a condenser. The electric displacement passes through the aperture of the iron annulus. Any changes in that displacement set up magnetic forces acting round the iron annulus, which, thereby, is subjected to a varying magnetisation. The annulus in turn sets up induction currents in the copper wire that surrounds it, these induction currents being received and rendered audible in an ordinary telephone receiver. The condenser is connected to a Ruhmkorff coil which rapidly charges and discharges it. The sounds heard in the telephone receiver establish the reality of the magnetic action of the variations in the electric displacement.

The author points out that this device, which may be regarded as a new kind of proof plane for exploring varying electrostatic fields, is probably capable of other useful applications, such as the investigation of specific inductive capacities.

Presents, February 28, 1889.

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Two Photographs of Pencil Sketches of Gauss and Olbers, by the late Professor Listing. Sir G. B. Airy, F.R.S.

"An Investigation of a Case of Gradual Chemical Change: the Interaction of Hydrogen Chloride and Chlorate in presence of Potassium Iodide." By W. H. PENDLEBURY, B.A., late Scholar of Christ Church, Oxford, Assistant Master of Dover College, and MARGARET SEWARD, late Tutor of Somerville Hall, Oxford, Science Lecturer of Holloway College. Communicated by A. VERNON HARCOURT, F.R.S. Received November 27,—Read December 13, 1888.

The work which we now have the honour of laying before the Royal Society was undertaken at the suggestion of Mr. A. Vernon Harcourt. To him we owe more than we can express, and we desire here to thank him most heartily for his most valuable aid and co-operation, by which many rough places in the investigation have been made smooth. We also thank the Royal Society for a grant in aid of the research, and the Governing Body of Christ Church for the use of materials and apparatus.

When substances which act upon each other are brought together under suitable conditions, a change takes place which consists in the disappearance of the original substances and the production in their place of an equal weight of other substances. The change proceeds till the whole of that reacting substance which was present in the smallest relative quantity has disappeared. This process may take a long time, as in the case which forms the subject of the present investigation, or the limit may be reached so rapidly that the change seems instantaneous. This difference, however, is one of degree and not of kind. In the present case the masses of the substances mixed together were so large relatively to the masses undergoing change during the time over which the observations extended, that the masses of reacting substances were practically constant. Thus it happens that each set of observations was of a change proceeding with constant velocity. In the second reaction studied by Messrs. Harcourt and Esson—

$$H_2O_2 + 2HI = 2H_2O + I_2,$$

the amount of change occurring during each interval of time in which it was estimated was a considerable fraction of the total amount of potential change, as limited by the amount of hydrogen dioxide taken. In this case, therefore, the observed intervals of time lengthened, being the time required for the performance of the same amount of chemical work with a continually diminishing amount of active substance.

The measurement of these intervals of time (whether constant or increasing) during which the same amount of chemical decomposition takes place, can be effected by taking advantage of a comparatively instantaneous change which may be made to go on in the same liquid, and one which is very familiar.

When iodine is produced in a liquid by the action of hydrogen dioxide, or some other oxidising agent, on hydrogen iodide, the action is a gradual one, but the introduction of a drop of a concentrated solution of sodium thiosulphate at once converts the iodine into sodium iodide, and every molecule of iodine produced in the liquid after the introduction of the drop will be instantly thus converted until the thiosulphate present is exhausted. If a small quantity of starch is present in the solution, the moment at which the last trace of thiosulphate disappears will be signalised by the appearance of a blue colour in the liquid, the effect of the free iodine upon the starch. Thus, then, by the introduction of constant measured quantities of sodium thiosulphate, the rate of progress of the action between potassium or hydrogen iodide and some oxidising substance may be readily measured. This is the principle and method of division into intervals in Messrs. Harcourt and Esson's classical research, and we have adopted it for the investigation of a similar case of chemical change.

The reaction chosen for investigation in the present case was one which liberated iodine indirectly. When solutions of potassium chlorate and hydrogen chloride are mixed together, the mixture soon acquires a chlorous smell, and at once liberates iodine from potassium iodide, and as time goes on continues to liberate more. The exact nature of the primary reaction, producing the oxidising agent, has not been ascertained, nor whether the product is chlorine, some oxide of chlorine, or a mixture of both. Various reactions are possible. The one fact which is certain seems to be that in presence of an iodide each molecule of chlorate salt is reduced to the corresponding chloride entirely and without intermediate stages, and the equivalent in iodine of all three atoms of oxygen set free. If it were the case that the decomposition of the chlorate molecule took place in stages there would be observed a considerable variation in the intervals depending on the amount of intermediate products present, which was not the case.

A mixture of hydrogen chlorate and hydrogen chloride, both dilute, reacts exactly in the same way as the mixture above, slowly producing oxidising material which liberates iodine from potassium iodide. It is probably a reaction common to most soluble chlorates. Part of the investigation has been concerned with such mixtures of the two acids without any metallic salt. These have advantages, as the reaction is not complicated by the presence of such salts. But

potassium chlorate was more often employed, being easily obtained and kept in a state of purity.

Bunsen gives the following hypothetical equations for a reaction between hydrogen chloride and any chlorate (the equations are given with hydrogen chlorate, for simplicity). But in these the number of molecules of chlorate reacting is arbitrarily limited to two. Without this limitation, it is obvious that the list of possible reactions may be indefinitely extended.

Relative proportion.		
1 : 1	......	$2HClO_3 + 2HCl = Cl_2O + Cl_2O_3 + H_2O$.
1 : 2	......	$2HClO_3 + 4HCl = 3Cl_2O + 3H_2O$.
1 : 3	......	$2HClO_3 + 6HCl = 2Cl_2O + 2Cl_3 + 4H_2O$.
1 : 4	......	$2HClO_3 + 8HCl = Cl_2O + 4Cl_2 + 5H_2O$.
1 : 5	......	$2HClO_3 + 10HCl = 6Cl_2 + 6H_2O$.

In our experiments the quantity of reacting substances was always such that, except for change in sodium thiosulphate, the composition of the mixture was sensibly the same at the end of the experiment as at the beginning. Each experiment was not carried to any definite limit, but was concluded as soon as the constant velocity of change in the mixture had been ascertained by the observation of several intervals corresponding to successive additions of thiosulphate.

The following considerations show the constancy of the composition of the mixture throughout an experiment. Each drop of thiosulphate corresponded, on an average, to the decomposition of three-millionths of a gram of potassium chlorate in each cubic centimetre of the mixture. Now, the smallest amount of potassium chlorate ever used was 0·01263 gram in each cubic centimetre, and of this only 0·000003 gram would have disappeared when as many as 10 drops of sodium thiosulphate had been added. This is an alteration of about 0·02 per cent. Or, to state it otherwise, in the case of one of the greatest velocities observed, when each interval was hardly greater than a minute, there was 0·03788 gram potassium chlorate in each cubic centimetre, and this was disappearing at the rate of 1·826-millionths of a gram per minute. Speaking roughly, it would take about 24 hours, proceeding at this rate, to cause a difference of 1 per cent. in the amount of salt present.

Messrs. Harcourt and Esson represented the variation of the intervals they observed with the mass present, y, as a logarithmic curve with asymptote meeting it when $y = \infty$. The constant intervals obtained in the present investigation would be represented in a portion of the curve produced to a great distance in the direction of the asymptote, this portion being sensibly a straight line parallel to the asymptote, so that the time observed for each interval is constant.

In our ordinary mode of working the reaction between chlorate and chloride occurred in presence of iodide. In order to examine the reaction when only chlorate and chloride were present and the products of their reaction were not at once reduced, and thus removed, the following experiments were made. Through a vessel containing a mixture of hydrogen chloride and potassium chlorate, kept at a constant temperature of 30°, a certain volume of air could be drawn at a fixed rate. The air, thus charged with a part of whatever gas was liberated in the mixture, was drawn through a series of washing-tubes containing potassium iodide. The liberated iodine was determined at the end of equal intervals of time. It was found that comparatively little oxidising gas was evolved. At the end of 20 hours the amount of gas dissolved in the mixture, capable of liberating iodine, was determined, and this quantity also was found to be very small. The remarkable diminution in the rate of formation of oxidising substance when no iodide was present will be evident when it is stated that whereas, in the presence of iodide, the change proceeded at such a rate that in 20 hours the amount of iodine set free would have corresponded to 6700 c.c. of the standard thiosulphate, the oxidising material formed in absence of an iodide only set free iodine corresponding to 100 c.c. thiosulphate.

The action was also found to be reversible in sunlight. Some of the mixture of potassium chlorate and hydrogen chloride, which had acquired a deep yellow colour, was exposed for a short time to bright sunlight; the solution became colourless, and was found to liberate no iodine. In our experiments, in presence of an iodide, we found sunlight to have no effect upon the rate of change.

It would thus appear that when the oxidising substance is produced in presence of an iodide it does its oxidising work at once and is removed, and the change proceeds uniformly. In the absence of an iodide, however, the oxidising substance accumulates in the liquid, and its further production is impeded probably by the occurrence of a reverse action.

Though the potassium iodide appears thus to be a necessary ingredient of the mixture if the change is to proceed at a uniform rate, it does not take part in the primary reaction; for otherwise variation in the amount of potassium iodide in the mixture, other things being unaltered, would produce very marked differences in the rate. That this is not the case was proved by an experiment, the results of which are shown in the following table:—

Table A.

n.	R.	n.	R.	n.	R.
1	61·30	5	66·18	9	65·98
2	63·20	6	65·93	10	65·93
3	64·42	7	66·10	11	66·10
4	65·50	8	66·40	12	66·73

In this table are given the rates, representing the number of hundred millionths of a gram of potassium chlorate decomposed per minute in each cubic centimetre of the liquid. The mixture contained in each cubic centimetre

$$\begin{cases} 0{\cdot}03789 \text{ gram potassium chlorate.} \\ 0{\cdot}02496 \text{ gram hydrogen chloride.} \end{cases}$$

The potassium iodide present during successive intensities was $n \times 0{\cdot}000001978$ gram, and n varied from 1 to 12.

It will be noticed that there is at first a slight acceleration with increase of potassium iodide, but very far from proportional to the increase, as would be the case if the reaction depended primarily on the amount of iodide present. After $n = 5$ the further multiplication of the small quantity produced little if any change in the rate. Perhaps the minute amount of iodide present during the first few observations was insufficient for the immediate amount of the chlorine and chlorous oxides formed.

In fact the above numbers, besides showing that the liberation of iodine is a separate reaction, not the primary one, seem also to indicate that though with the quantity of potassium iodide usually taken this secondary reaction *is* instantaneous compared with the primary one, if the quantity is much decreased the former does take up a time which is comparable with that of the latter, and so may produce an appreciable retardation.

We shall return to the consideration of variation of potassium iodide later on, but have pointed this out to emphasise further the observation already made, that time must be a factor in all changes, but in very few does the connexion come within our powers of observation, so that other changes compared with these few are called instantaneous.

The amount of potassium iodide generally used in our experiments corresponds to $n = 60$ in this series. In other sets of observations some of which are recorded in Tables XI, XII, and XIII, p. 417—419, the effect of adding larger amounts of potassium iodide was tried. The

effect is to produce a slight addition to the rate, proportional to the amount of iodide added. A similar result has been obtained with potassium chloride, and in view of these results we conclude that potassium iodide acts only as an indifferent salt, and does not immediately promote the reduction of the potassium chlorate, but only serves to prevent the accumulation in the liquid of chlorine or chlorine oxides precisely as the presence of thiosulphate serves to prevent the accumulation of iodine.

FIG. 1.

The apparatus employed in all our experiments was the same as that used by Messrs. Harcourt and Esson, and consisted of a cylinder of white glass 310 mm. high and 64 mm. in diameter; at a distance of 213 mm. from the base a fine line was etched round the cylinder marking a volume of 792 c.c. The cylinder was closed with an india-rubber stopper perforated with three openings, through which passed a thermometer and an inverted funnel tube. The third hole was ordinarily closed with a cork, and served to give access to the contents of the cylinder. The inverted funnel tube was connected with an apparatus for the generation of carbon dioxide.

The method of proceeding was as follows:—Into the cylinder previously filled with carbon dioxide was brought the weighed quantity of potassium chlorate to be employed, with sufficient water to dissolve it. To this was added a measured volume of hydrogen chloride of

known strength, together with 10 c.c. of potassium iodide solution containing 0·1 gram of the salt and 10 c.c. of clear starch solution, mixture being rapidly made after each addition by the passage of large bubbles of carbon dioxide from the inverted funnel. These bubbles of gas, with a diameter equal to half that of the cylinder, served to stir the liquid and also to exclude the air. A few drops of a dilute solution of thiosulphate were added to keep down the blue colour till all was prepared for observation. There was a line scratched on the funnel stem, and this mark and one of the graduations of the thermometer were made to coincide with the plane of the line round the cylinder. The temperature of the liquid was brought up to the required point, then the cylinder was placed on a levelling stand, and water was added till the lower surface of the meniscus just coincided with the plane of the marked line. Meanwhile a number of small measures of a concentrated solution of sodium thiosulphate had been prepared. These measures must be equal or have a known ratio to each other; they must also be of small volume, in order that their addition may not materially affect the dilution of the liquid. These measures were obtained in the following way:—A series of tubes about 8 inches long, having a lateral orifice about $1\frac{1}{2}$ inches from the end, such as would be made for the purpose of joining on another tube at right angles, were mounted on a carriage, each tube having a separate rest, and all the orifices being in one line. By the turn of a screw connected with a rack and pinion these tubes could be brought exactly under a siphon delivering drops of thiosulphate. The siphon and its reservoirs stood on a bracket attached to a pillar of solid masonry to prevent vibration. The whole was enclosed in a glass case like that of a balance, the front of which was shut down during the time of collection of the drops. The time of formation of a drop was generally about half a minute. The width of the reservoir containing the thiosulphate is so great in comparison with the quantity of solution taken for any one set of experiments that the available length of the siphon and the rate of flow, upon whose constancy that of the drops depends, varies in no appreciable degree. At the end of each experiment the value of the drops employed was determined by means of a standard iodine solution.

When the observations were to be made the cylinder was placed on a sheet of white paper in a good light, opposite a clock beating seconds. The paper lay on an iron plate, which could be heated at once, and by a lamp if necessary, and thus the cylinder could be kept at any desired temperature by moving it nearer to or further from the heated end of the plate. When once the most convenient spot has been selected, a mere touch with the hand was all that was required to maintain the temperature constant.

The observations were made by looking down on the column of fluid and watching the appearance of the disk forming its upper surface. As soon as the change is complete a blue shade shoots rapidly across the brightly illuminated disk, and there is no difficulty in ascertaining the exact second of the change; the observer listens to the beat of the clock and counts the seconds whilst watching the disk. As soon as the blue colour has appeared, the minute and second are noted, and a drop of thiosulphate is brought into the cylinder. The end of the tube charged with a drop is plunged into the liquid through the opening for that purpose, and moved up and down, active stirring being carried on by means of the bubbles of carbon dioxide. The intervals date from one appearance of the colour to the next reappearance, and as the rate is not affected by the presence of a small amount of iodine or a small diminution in the amount of iodide, it is clear that the fact of the addition and admixture of the thiosulphate not following immediately upon the appearance of the blue colour, does not disturb the uniformity of the rate of change.

The potassium chlorate employed in our experiments was purified by recrystallisation.

The hydrogen chlorate was prepared by the cautious addition of sulphuric acid to a solution of pure barium chlorate until no milkiness was produced either by further additions of hydrogen sulphate or barium chlorate. The barium sulphate precipitate was then filtered off. The solution of hydrogen chlorate thus obtained contained no chloride.

The potassium iodide solution was prepared by dissolving 80 grams of recrystallised and fused potassium iodide in 8 litres of water.

The starch solution employed was prepared by adding a magma of starch and water, containing about 5 grams of starch to about 300 c.c. of boiling water, and allowing the whole to boil briskly for a few minutes. When cool, the liquid was transferred to a cylinder and covered over. On standing, the upper part of the liquid become perfectly clear; of this 10 c.c. were taken by means of a pipette.

The hydrogen chloride solution was prepared by diluting pure acid till 100 c.c. contained 18·823 grams of hydrogen chloride.

It may be of interest here to give the details of an actual experiment.

Taken—30 grams of potassium chlorate, 200 c.c. hydrochloric acid (containing 18·823 grams hydrogen chloride in 100 c.c.), 10 c.c. of a clear starch solution, and 10 c.c. of a solution of potassium iodide (containing 0·01 gram potassium iodide per c.c.). A few drops of a dilute solution of thiosulphate were added to discharge the colour due to the iodine liberated before it was possible to commence observations.

The temperature throughout was 20°.

As soon as all was ready the time of the first reappearance of the blue colour was noted, then a drop of thiosulphate was introduced as described, and the next appearance of colour was noted.

Table B.

Time.	Interval.	
	Mins. secs.	Mins. and decimals of a min.
I 38′ 24″		
I 40 2	1 38	1·63
I 41 41	1 39	1·65
I 43 20	1 39	1·65
I 44 59	1 39	1·65
I 46 37	1 38	1·63
I 48 15	1 38	1·63
I 49 54	1 39	1·65
		Mean 1·64

The value of each of the thiosulphate drops in standard iodine solution was then determined.

The equivalent of one drop is 6·05 c.c. of this solution, containing 0·00248 gram iodine per c.c. Now 1 millionth-gram-molecule of potassium chlorate liberates 7·62 millionth-grams in weight of iodine.

Therefore the number of millionth-gram-molecules of potassium chlorate decomposed in each cubic centimetre of the mixture per minute is

$$\frac{6{\cdot}05 \times 0{\cdot}00248}{762 \times 792 \times 1{\cdot}64} = 0{\cdot}01512 \text{ mgm.}$$

The quantities taken of the reacting substances represented

HCl ...... $20 \times 65{\cdot}11$ m.g.m. per c.c.
$KClO_3$ .... $30 \times 51{\cdot}5$,,

Scheme of the Paper.

In describing the results of our investigations we shall first consider the action of hydrogen chlorate on hydrogen chloride and examine the effect of variation of the former acid on the rate of change; we next consider the effect of variation in hydrogen chloride on such a mixture. We then consider and examine the effect of introducing potassium chloride into the mixture of the two acids, and from the results thereby obtained we gain considerable help in our

further investigation, viz., the action of hydrogen chloride on potassium chlorate.

In this reaction we examine the effect on the rate of variation in the amount of hydrogen chloride, the potassium chlorate being kept constant. We then briefly touch on the results obtained by varying the potassium chlorate.

We next consider the effect of variation in the amount of potassium iodide present, used as an indicator of the performance of a definite amount of chemical work.

We lastly discuss the effect of variation in the temperature at which the reaction of hydrogen chloride and hydrogen chlorate takes place.

Variation in Hydrogen Chlorate.

A series of observations were made in which the quantity of hydrogen chlorate was varied in arithmetical progression, the hydrogen chloride being kept constant. When each of the numbers representing velocity of decomposition was divided by a number representing the amount of hydrogen chlorate present, the series of numbers obtained was approximately an arithmetical progression, the difference being a small one. So that if u represents the hydrogen chlorate, R the rate of decomposition, a formula by which the latter may be calculated from the former is of the form

$$R = u(A+Bu),$$

where A and B are constants.

The following tables contain examples of the results obtained by experiment, and by calculation from this formula.

The rate = the number of times the following reaction in millionth of a gram-units takes place in each c.c. per minute.

$$6HCl+6KI+KClO_3 = 7KCl+3H_2O+3I_2.$$

$$HCl = 16\times 65{\cdot}11 \text{ millionth-gram-molecules in each c.c.}$$
$$HClO_3 = u\times 61{\cdot}3 \quad\text{,,}\quad\quad\text{,,}$$

u varies from 2 to 6.

Table I.

u.	Rate observed.	Rate calculated. $R = u(0{\cdot}00073+0{\cdot}000025u.)$
2	0·00246	0·00246
3	0·00434	0·00444
4	0·00695	0·00692
5	0·00996	0·00990
6	0·01320	0·01338

Table II.

$$HCl = 20 \times 65{\cdot}11 \text{ m.g.m.}$$
$$HClO_3 = u \times 61{\cdot}3 \text{ m.g.m.}$$

u.	Rate observed.	Rate calculated. $R = u(0{\cdot}00244 + 0{\cdot}00035u)$.
2	0·00628	0·00628
3	0·01028	0·01047
4	0·01542	0·01536
5	0·02090	0·02095
6	0·02858	0·02724

Table III.

$$HCl = 15 \times 65{\cdot}11 \text{ m.g.m.}$$
$$HClO_3 = u \times 61{\cdot}3 \text{ m.g.m.}$$

u.	Rate observed.	Rate calculated. $R = u(0{\cdot}00083 + 0{\cdot}00016u)$.
3	0·00393	0·00393
4	0·00578	0·00588
5	0·00800	0·00815
6	0·01074	0·01074

The connexion between the velocity of decomposition and the amount of the decomposing substances present is of exactly the same nature as that established in a similar way by Messrs. Harcourt and Esson, viz., that the velocity varies in the first place directly with the mass of the substance present, and that in the second place the presence of the substance causes a slight acceleration in the rate irrespective of its being decomposed. It has already been conclusively shown by their work and that of other experimenters on the same lines that the presence in the liquid of any substance which, as far as is known, has no chemical action upon the essential ingredients, and may therefore be considered to remain inactive during the change nevertheless has its specific effect, accelerating or retarding upon the velocity of the change. But Harcourt and Esson pointed out that the decomposing substance itself likewise exercised this secondary influence. It does so in this case, and the second term in the empirical formula represents this secondary effect.

Variation in Hydrogen Chloride.

Having satisfactorily established the relation of the rate of change to the amount of one of the reacting substances, namely, hydrogen chlorate, we naturally sought to find the connexion between the rate and the amount of hydrogen chloride—the other reacting substance. It might be expected that the effect of variation in the hydrogen chloride would result in equations of the same form as those given above. Various series of observations were made to investigate this point. The amount of acid was varied in arithmetical progression, and the rates obtained were divided by the quantity of acid present in each case to see if anything approaching an arithmetical progression could be obtained.

No such relation appears to exist, as is shown by the following example:—

Table IV.

$HCl = v \times 65{\cdot}11 - 3 \times 51{\cdot}5$ millionth-gram-molecules per c.c.
$HClO_3 = 3 \times 51{\cdot}5$ " "

v.	Rate observed.	$\dfrac{\text{Rate}}{v \times 65{\cdot}11 - 3 \times 51{\cdot}5}$.	Difference (which ought to be constant).
20	0·00739	644	86
19	0·00604	558	52
18	0·00516	506	61
17	0·00405	445	66
16	0·00331	379	69
15	0·00255	310	43
14	0·00202	267	42
13	0·00156	225	39
12	0·00117	186	30
11	0·00088	156	35
10	0·00060	121	

The first four or five numbers in the third column might perhaps be brought into an arithmetical progression without any serious alteration, but taken as a whole, the series of experimental results cannot be thus interpreted. It appears then that the effect of hydrochloric acid is not, like that of chloric acid, of two kinds, viz., (1) a primary one due to its being a decomposing substance, and (2) a secondary one of the nature above described. Yet it can hardly be supposed that it acts merely in a secondary way as a substance present and not decomposed, for its effect is proportionally much greater than that of chloric acid itself. Thus in the above series when the quantity of acid is only a little more than doubled ($v = 10$

and $v = 20$) the rate in the second case is about twelve times that in the former.

We have however proved by trial that chloric acid of itself, *without* hydrochloric acid, when mixed in the cylinder with the other ingredients, will evolve oxidising material. The rate is exceedingly slow :—

$$HCl = 0.$$
$$HClO_3 = 6 \times 51{\cdot}5. \qquad \text{Rate} = 0{\cdot}000000564.$$
$$\text{Temp.} = 20^\circ.$$

It is possible that two reactions are going on at the same time, one with chloric acid alone, the other substances present having merely their specific effect, and also the action between chloric acid and hydrochloric acid, both producing oxidising material.

Now amongst the various attempts made to find empirically the law of connexion between variation of hydrochloric acid and variation of rate, one result arrived at was that second differences of the rates are approximately constant. Especially is this noticeable for smaller quantities of acid. The first differences thus resemble an arithmetical progression. The next table consists of the same rates as in Table IV, compared with a series of numbers obtained by recalculation after substituting for the first differences of these a true arithmetical progression, being the one they most nearly approach. The constant difference in this case would be 0·000095. Beginning from $v = 11$, we get the following results :—

Table IV*b*.

Amount of HCl. v.	Rate observed.	Rate calculated.
20	0·00739	0 00689
19	0·00604	0·00584
18	0·00516	0 00489
17	0·00405	0·00403
16	0·00331	0·00327
15	0·00255	0·00260
14	0·00202	0·00203
13	0·00156	0·00155
12	0·00117	0·00117
11	0·00088	0·00088
10	0 00060	0·00068

From $v = 10$ to $v = 17$, the empirical numbers correspond fairly with the observed rates, but afterwards the latter increase more

rapidly. Now if two reactions of the nature above described are really taking place, it would lead us to conjecture a connexion expressed by the following equation:—

$$R = ku(1+\alpha u+\beta v)+k'uv(1+\alpha' u+\beta' v),$$

u and v representing as usual the quantities of hydrogen chlorate and hydrogen chloride present, the other letters constants.

This expression is of the form

$$R = A+Bv+Cv^2,$$

when v is the only variable, and a series of such rates for which v was varied in arithmetical progression would have its second differences constant. Possibly the coefficient β is negative, *i.e.*, the presence of hydrogen chloride interferes with and retards the decomposition of hydrogen chlorate by itself. This would explain why, in the rates obtained with larger quantities of hydrogen chloride ($v = 17$ to $v = 20$), the ordinary formula

$$R = k'v(1+\beta' v)$$

more nearly expresses the results obtained; the reason of this being that in the presence of a large quantity of this acid the other reaction may be altogether stopped. All this, however, is conjectural. A second series obtained could not be brought into partial agreement with the formula above; yet other mixtures of potassium chlorate and hydrogen chloride gave series of numbers of exactly the same character as this first one. These series we shall give later. The variation of the rate with the amount of hydrogen chloride present is evidently by no means a simple one. The interpretation of its complications that we have suggested can scarcely be considered fully established. It would, however, account for the facts observed.

Since in the main reaction which we desired to study, viz., that between potassium chlorate and hydrogen chloride, there would be produced during the reaction some amount of potassium chloride by the decomposition of the chlorate, we determined the effect of the addition of potassium chloride to the mixture of the two acids, hydrogen chlorate and chloride.

We have already referred to the fact that in gradual reactions, such as the present, substances which remain in the solution apparently unchanged throughout the whole reaction yet exercise their specific influence, accelerating or retarding, on the velocity of the change, hence it becomes important to ascertain the effect of the potassium chloride. When the potassium chlorate and hydrogen chloride are mixed together, the latter being always in some excess, there is double decomposition, and potassium chloride and hydrogen chlorate

are formed. If the action is a complete one, all the potassium chlorate will be converted into chloric acid and potassium chloride formed in corresponding amount. There is a good deal of evidence in favour of the completeness of the decomposition in the cases we have investigated. Indeed it is perhaps to be expected *a priori* that when a stronger acid, such as our hydrochloric, is in great excess, it might entirely turn the weaker acid out of combination. If this be the case, it follows that in this reaction also the reacting substances are, as before, chloric and hydrochloric acid, and that potassium chloride is present as a "neutral" substance. At any rate, however, this compound is present to some extent in the mixture. To determine its effect mixtures of hydrogen chlorate and chloride were made, and to them quantities of potassium chloride in arithmetical progression were added, and the effect on the rate observed. The following tables show the results obtained:—

Table V.

	w.	R.	Difference.
$HClO_3 = 6 \times 51{\cdot}5$ millionth-gram-mols.	0	0·00252	
$HCl = 13 \times 65{\cdot}11$,,	2	0·00281	0·00029
$KCl = w \times 51{\cdot}5$,,	4	0·00306	0·00025
	6	0·00333	0·00027

Table VI.

	w.	R.	Difference.
$HClO_3 = 3 \times 51{\cdot}5$ millionth-gram-mols.	0	0·00335	
$HCl = 16 \times 65{\cdot}11$,,	2	0·00357	0·0022
$KCl = w \times 51{\cdot}5$,,	4	0·00382	0·0025

The effect of potassium chloride in the mixture is thus an accelerating one, and takes place in accordance with the formula already mentioned, the rates increasing in arithmetical progression approximately as the quantity of salt present is similarly increased. If R_w is the rate with a quantity w of potassium chloride,

$$R_w = A(C + aw),$$

where A and C are quantities independent of w; and a is the coefficient of action; and $A \times C = R_0 =$ rate without potassium chloride.

Moreover, the addition of the potassium chloride appears to have no such disturbing effect as would result if potassium chlorate was formed to some extent as soon as the potassium chloride was added, and a condition of saline equilibrium between four substances resulted. In the first series in the above table the mixture in its last stage corresponded exactly to a mixture of potassium chlorate (6×51.5 millionth-gram-molecules) and hydrochloric acid (13×65.11) supposing that complete double decomposition had taken place. For comparison, therefore, a mixture was made containing initially these amounts of potassium chlorate and hydrochloric acid with this result:—

$$\text{Rate obtained} = 0.00337.$$
$$\text{Rate in table} = 0.00333.$$

This result might, of course, be taken merely to mean that the same state of saline equilibrium had been attained in both cases, but it has been already pointed out that the effect of progressive additions of potassium chloride, giving a result expressible by a formula like the above, is to show that it remains an unaltered substance in the mixture.

In the experiment detailed below, the salt was added to a mixture made with potassium chlorate and hydrochloric acid, and therefore it is presumed that it contained already some potassium chloride, obtained by saline decomposition. The results then obtained were of the same nature as before:—

Table VII.

	w.	R.	Difference.
$KClO_3 = 2 \times 51.5$.	0	0·00354	0·00018
$HCl = 16 \times 65.11 - 2 \times 51.5$.	2	0·00372	0·00022
$KCl = w \times 51.5$.	4	0·00394	0·00021
	6	0·00415	

It will be seen that the salt added continues to have its specific accelerative effect, and though at the end the whole quantity of potassium chloride present was 8×5.15 millionth-gram-molecules, there is no sign whatever of the saline equilibrium being upset. The quantity of hydrochloric acid present is about double this ($16 \times 63.11 - 2 \times 51.5$).

In all these experiments, the highest precision in adding the potassium chloride was not possible, as it was necessary to add the solid salt to a liquid of standard volume, and a slight variation of the conditions of the experiment besides the one contemplated was thus inevitable.

But we think there can be no doubt of the truth of the important conclusion we make from these experiments, that in all the mixtures we have made with potassium chlorate and hydrogen chloride (the molecular ratio varying from about 1 : 2 to 1 : 12) there is complete and immediate double decomposition, leaving in the mixture potassium chloride, hydrogen chlorate, and excess of hydrogen chloride; and that the reaction producing oxidising material takes place between the two acids alone.

For the facts are briefly these. Corresponding to each mixture of potassium chlorate and hydrogen chloride, we may make a mixture containing of hydrogen chlorate the amount corresponding in molecular weight to the potassium chlorate, and of hydrogen chloride the amount as before less the quantity required to decompose the potassium chlorate. Then the rate in this second mixture will be a little slower than that in the first. If now the amount of potassium chloride corresponding molecularly to the potassium chlorate be taken, divided into a small number of equal quantities, and these added separately to the second mixture, the rate will increase by an equal quantity for each addition (as upon the introduction of any neutral salt), until when all has been added the rate is approximately the same as that of the first mixture.

The following are further examples of the correspondence between the two sorts of mixtures :—

Table VIII.

I.	$HClO_3 = 6 \times 51{\cdot}5$ millionth-gram-molecules per c.c.	
	$HCl = 18 \times 65{\cdot}11 - 6 \times 51{\cdot}5$.	
	$KCl = 6 \times 51{\cdot}5$.	
		Rate = 0·0105.
	$KClO_3 = 6 \times 51{\cdot}5$.	
	$HCl = 18 \times 65{\cdot}11$.	
		Rate = 0·0104.
II.	$KClO_3 = 6 \times 51{\cdot}5$.	
	$HCl = 15 \times 65{\cdot}11$.	
		Rate = 0·00554.
	$HClO_3 = 6 \times 51{\cdot}5$.	
	$HCl = 15 \times 65{\cdot}11 - 6 \times 51{\cdot}5$.	
	$KCl = 6 \times 51{\cdot}5$.	
		Rate = 0·00555.
III.	$KClO_3 = 2 \times 51{\cdot}5$.	
	$HCl = 15 \times 65{\cdot}11$.	
		Rate = 0·00195.
	$HClO_3 = 2 \times 51{\cdot}5$.	
	$HCl = 15 \times 65{\cdot}11 - 2 \times 51{\cdot}5$.	
	$KCl = 2 \times 51{\cdot}5$.	
		Rate = 0·00191.

We are now in a position to discuss the results obtained in the investigation which Mr. Harcourt originally proposed that we should make, viz., the action of hydrogen chloride on potassium chlorate.

We shall first discuss the results obtained by varying the hydrogen chloride, keeping the potassium chlorate fixed. The hydrogen chloride varied from $v = 20$ to $v = 10$. After the double decomposition mentioned, the amount of acid present is $v \times 65{\cdot}11 - u \times 51{\cdot}5$. As u is constant, the acid varies in arithmetical progression. In the following table $u = 3$, and thus corresponds to the chloric acid results in Table IV as far as the amounts of acid go, and only differs from it in having present a certain quantity of potassium chloride. It is, therefore, to be expected that the variation will be of the same nature, and this we find to be the case.

Table IX.

v.	Rate.	$\dfrac{\text{Rate}}{v \times 65{\cdot}11 - 3 \times 51{\cdot}5}$. (Dec. points omitted.)	Difference
20	0·00876	764	
			93
19	0·00725	671	
			96
18	0·00585	575	
			71
17	0·00480	504	
			81
16	0·00374	423	
			52
15	0·00305	371	
			48
14	0·00245	323	
			47
13	0·00191	276	
			38
12	0·00149	238	
			42
11	0·00110	196	
			34
10	0·00081	162	

By inspection it will thus be seen that the third column is not an arithmetical progression. If, however, we treat the series in the other way, we find the second differences of the rate to be approximately constant. Then substituting for the first differences the nearest exact arithmetical progression and re-calculating the rates, we get a series in which the calculated and observed numbers agree fairly well between $v = 16$ and $v = 10$, just as in the corresponding chloric acid series given in Table IV (*b*).

Table IX*b*.

Amount of hydrogen chloride. *v*.	Rate observed.	Rate calculated.
20	0·00876	0·00731
19	0·00725	0·00630
18	0·00585	0·00537
17	0·00480	0·00452
16	0·00374	0·00375
15	0·00305	0·00306
14	0·00245	0·00245
13	0·00191	0·00192
12	0·00149	0·00147
11	0·00110	0·00110
10	0·00081	0·00081

The next table contains the results obtained in a series when $u = 4$ and v varied as before, the calculated rates are obtained in a way similar to the last, as the second differences were again approximately constant:—

Table X.

v.	Rate observed.	Rate calculated.
20	0·01213	0·01151
19	0·00989	0·00977
18	0·00811	0·00819
17	0·00677	0·00677
16	0·00549	0·00551
15	0·00439	0·00441
14	0·00341	0·00347
13	0·00266	0·00269
12	0·00207	0·00207
11	0·00161	0·00161
10	0·00117	0·00121

The numbers here again coincide fairly well except for the highest values of *v*, and this is consistent with the theory that the decomposition of chloric acid by itself is checked when the quantity of hydrochloric acid is large, for here we have a larger quantity of chloric acid produced than we had before, and a larger amount of the hydrochloric acid is required before the decomposition of the chloric acid alone is checked by the latter.

We have obtained a series in which the amount of potassium chlorate employed was as high as six units. Here, as with the case of the corresponding quantity of chloric acid, no approach to an interpretation could be attained.

All these things show that the effect of varying hydrochloric acid with chloric acid or with potassium chlorate is the same, though of what exact nature that effect is, we have not yet fully determined. No doubt the rate obtained for the decomposition of chloric acid alone is too slow to account satisfactorily for the numbers not following a law similar to that for variation in chloric acid first established.

Series of experiments were made in which the amount of potassium chlorate used was alone varied, the hydrochloric acid being constant as regards the amount added each time. It will be seen, however, from the potassium chloride results that we were not varying the potassium chlorate only in this case, but really were varying both this salt and the acid. For after saline decomposition—

$$\begin{aligned} HClO_3 \text{ per cent.} &= u \times 51{\cdot}5. \\ HCl \qquad\qquad &= v \times 65{\cdot}11 - u \times 51{\cdot}5. \end{aligned}$$

The variation of rates in these series, therefore, must follow a very complicated law. We have, however, drawn a series of curves, representing the variation of rate in this part of the investigation (p. 416).

The curves are thus drawn:—

A series of equidistant base lines (marked by broken lines) are taken, one base line corresponding to each quantity of hydrogen chloride used, and therefore marked at the extremity with a number representing that quantity.

Along these base lines are marked off lengths corresponding to the quantities of potassium chlorate taken, and then lengths representing the rates are measured perpendicular to these. The distance between two blue lines represents—

$$0{\cdot}00018\ (6HCl + 6KI + KClO_3 = 7KCl + 3H_2O + 3I_2) \text{ in millionth grams.}$$

The lower curves cross the base lines above them, but this does not interfere with their comparison with the others.

It is worth observing, however, that in nearly all the cases tried, R/u decreased instead of increasing with u, after $u = 4$ or after $u = 5$; owing of course to the fact that the amount of hydrochloric acid is decreasing as u increases. In fact it may be inferred that dR/du has a root between 4 and 6.

We now turn our attention to the behaviour of another constituent of our mixture, viz., potassium iodide. We spoke of the part it played in our experiments in the introduction to our paper, but now

Fig. 2.

Curves representing the effect on the rate of variation of $KClO_3$. Each curve represents a certain quantity of HCl present.

we come to consider more particularly its action and the effect of its variation on the rate of decomposition.

Variations in Potassium Iodide.

It has been already pointed out that it is essential to the uniformity of the rate of change that there should be potassium iodide present, for in our preliminary experiments we showed that the reaction was stopped if the oxdising material was allowed to accumulate in the liquid instead of being removed by its reaction, with potassium iodide. At the same time there is no evidence to prove that potassium iodide takes part in the primary reaction. If we examine a few cases in which the amount of iodide was varied we shall soon see what sort of influence it exercises. The quantity used in all our experiments was as a rule 0·76 millionth-gram-molecule per c.c., a very small quantity in proportion to the other ingredients.

In the following table, in the initial experiment the amount used was very much smaller, 0·00946 m.g.m., and similar quantities were added one by one and their effect upon the rate ascertained.

Table XI.

	x.	R.
$HCl = 15 \times 65{\cdot}11$.	1	0·00500
$KClO_3 = 6 \times 51{\cdot}5$.	2	0·00516
$KI = x \times 0{\cdot}00946$.	3	0·00525
	4	0·00534
	5	0·00540
	6	0·00538
	7	0·00539
	8	0·00543
	9	0·00538
	10	0·00538
	11	0·00540
	12	0·00544

The rate in the same mixture in the ordinary experiments when the usual quantity (0·76 m.g.m.) of potassium iodide was introduced was 0·00554.

It thus appears that when the quantity of the substance present initially is very small, doubling the amount produces a marked increase of the rate, but after a certain amount has been added, further small quantities produce no marked result. Such a series then does not correspond to the ordinary form of variation with quantity of neutral salt, but one would be led to infer that if we call 0·0054 the normal rate we shall only get this rate when the amount of iodide present is

great enough, a retardation following any diminution of the iodide beyond this minimum necessary, and our experiments fully bear this out. The reason probably is that there is a tendency for molecules of oxidising material to begin to accumulate in the liquid if they do not immediately find molecules of potassium iodide to react with. In other words, whereas we are accustomed to consider the second reaction between chlorine or oxides of chlorine and potassium iodide, to be instantaneous, this is true only when the amount of potassium iodide present is beyond a certain minimum. In the mixture above the minimum is between 0·03784 m.g.m. and 0·0473 m.g.m. per c.c., and after that the rate of decomposition remains practically stationary until the amount present is 0·11352 m.g.m. per c.c.

The following table shows the effect of variation of iodide by larger quantities at a time, beginning with about half the usual quantity 0·76 m.g.m. per c.c.

Table XII.

	x.	R.
$HCl = 11 \times 66{\cdot}11$.	1	0·00378
$KClO_3 = 6 \times 51{\cdot}5$.	2	0·00389
$KI = x \times 0{\cdot}367$.	3	0·00392
	4	0·00391
	5	0·00393
	6	0·00401
	7	0·00403
	8	0·00403
	9	0·00407
	10	0·00409
	11	0·00416

There is a slight increase at the beginning of the series when the quantity of iodide is doubled; after this the rate remains practically stationary for several increments. There is, however, a marked increase between $x = 3$ and $x = 11$, but not so rapid as at the beginning of the series. The reason for the indistinctness of form is evidently the fact that the effect of variation of the iodide is within the limits of experimental error. It was, therefore, deemed advisable to vary the iodide by larger quantities. In the following series single grams of the substance were introduced into the cylinder, one after the other.

Table XIII.

	s.	R.	R calculated as arithmetical progression.
$HCl = 15 \times 65{\cdot}11$. $KClO_3 = 6 \times 51{\cdot}5$. $KI = s \times 7{\cdot}6$.	1	0·00661	0·00661
	2	0·00736	0·00725
	3	0·00786	0·00789
	4	0·00871	0·00853
	5	0·00932	0·00917
	6	0·00980	0·00981
	7	0·01043	0·01045

Here we have a series in which the rate increases distinctly with the amount of iodide, and it is not far from an arithmetical progression, certainly within the limits of experimental error. The establishment of this relation would of course show that the variation of potassium iodide has the same sort of influence as any neutral salt, and one would therefore class it with potassium chloride in this investigation. On the other hand, it seems evident that the cases are not exactly parallel; double decomposition between this salt and hydrogen chloride must go on, with a production of potassium chloride and hydrogen iodide. The hydrogen chloride present is decreased by a quantity equivalent to the amount of potassium chloride produced, and the hydrogen iodide produced will have its specific influence different from that of the former acid. At any rate one cannot be surprised at not finding the progression quite as well marked here as for potassium chloride.

We will now turn to the results obtained by varying the temperature at which we made observations. This was done in the manner described in the introduction.

Variation in Temperature.

The temperature at which most of our experiments were conducted was 20° C. We found any variation in temperature had a marked effect on the rate. A rise of temperature of 5° practically doubled the rate of decomposition, and a fall in temperature of 5° halved the rate at any point. In general if the temperature was varied in arithmetical progression the rate varied in geometrical progression. The following tables illustrate this fact:—

Table XIV.

	t.	R.	Ratio.
I $KClO_3$ = 6 × 51·5. HCl = 14 × 65·11.	15°	0·00215	
	20	0·00427	1·98
	25	0·00838	1·96
	30	0·01641	1·96
II. $KClO_3$ = 6 × 51·5. HCl = 19 × 65·11.	4	0·00136	
	7	0·00213	1·57
	10	0·00330	1·55
	13	0·00509	1·54
III. $KClO_3$ = 6 × 51·5. HCl = 10 × 65·11.	20	0·00143	
	21	0·00164	1·149
	22	0·00186	1·134
	23	0·00213	1·145

We see from the above tables that the ratios vary with the differences between the successive temperatures. They are, however, quite independent of the quantity of each substance present. This is clearly shown in the following table, in which the rates obtained with varied quantities of potassium chlorate at 25° and 30° are compared. It will be seen that the ratio is constant. A similar result was obtained with varied quantities of potassium iodide, the ratios in this case also being independent of the amount.

Table XV.

	n.	Rate at 25°.	Rate at 30°.	Ratio.
HCl = 10 × 65·11. $KClO_3$ = n × 51·5.	3	0·00162	0·00319	1·974
	4	0·00206	0·00404	1·957
	5	0·00255	0·00502	1·966
	6	0·00301	0·00584	1·942
	7	0·00334	0·00650	1·957

It will be seen that the rate almost doubles itself for the rise of 5° between 25° and 30° in the above instances.

A great number of experiments were made in which the temperature was varied by successive differences of 3°, or 2°, or 1°; but the numbers observed could not be considered quite satisfactory owing to the intrusion of experimental errors, so it was generally found advisable to increase the temperature 5° at a time, thus making a marked difference in the rate. We deduce from this result the average value of the coefficient or ratio for the variation in temperature of 1°.

The formula embodying these results takes the same form as that in Messrs. Harcourt and Esson's reaction, in which it may be remembered the rate of decomposition doubled itself for a difference of 10° in temperature, whilst in our reaction it is doubled for every 5°.

The formula is thus expressed :—

$$R_t = R_0 e^{kt},$$

where t is temperature, R_0 rate at 0°, R_t is rate at t°, k is a constant. The rough approximation that the rate doubles itself for 5° would give $k = 0{\cdot}3010/5 = 0{\cdot}0602$. The value of k is determined from experiment as—

$$\underline{\log R_t - \log R_{t-5}}$$

and the mean of a large number of experiments gives it as about 0·0585.

k is, however, not absolutely constant, but is found to vary slightly with the temperature (t) for which it is determined. It is larger for a low temperature range of 5°, and smaller for a difference of 5° at a higher temperature. In fact, speaking roughly, between 0° and 15° the rate is a little more than doubled by a rise of 5°; between 20° and 35° it is a little less than doubled. The following table will show the amount of variation from this ratio :—

Table XVI.—Values of k between—

0° and 5°.	5° and 10°.	10° and 15°.	15° and 20°.
0·0643	[0·0599]	0·0610	0·0588
0·0658		0·0609	0·0605
			0·0595
20° and 25°.	**25° and 30°.**	**30° and 35°.**	**35° and 40°.**
0·0584	0·0584	0·0537	0·0508
0·0576	0·0592	0·0547	
0·0580	0·0583		
0·0580			
0·0586			
0·0590			
[0·0566]			

k is thus seen to vary slightly with the temperatures between which it is determined. The same secondary variation was noticed by Messrs. Harcourt and Esson in their reaction. On comparing column 4 with column 5 it will be seen that their mean value is almost the same. At present it is difficult to extricate the secondary variation from experimental error, especially as a greater range of temperature

cannot be taken. At temperatures above 35° the starch-iodide colour is very difficult to perceive, as it loses its distinctive blue tinge and acquires a purple colour. At temperatures below 0°, though the starch colour is then a most beautiful blue, yet the change proceeds so slowly that it becomes difficult to hit, even within a few minutes, the point at which the blue colour has definitely appeared. Hence the range of temperature is somewhat limited.

This brings our work to a conclusion. There are several points which still need elucidation; their interpretation has seemed, so far, beyond our powers. We can only add a few facts to the pile now rapidly accumulating, out of which should grow a comprehensive theory of chemical dynamics.

The facts established by the investigation may be thus summarised:—

Dilute solutions of hydrogen chlorate and hydrogen chloride when mixed together slowly liberate oxidising material, chlorine and oxides of chlorine.

If no substance which can be oxidised is present, the accumulation of this oxidising material in the liquid soon stops the reaction.

In the presence of an iodide from which iodine can be liberated, and afterwards disposed of by means of sodium thiosulphate, the reaction proceeds regularly and with a constant velocity—constant because the quantity of the substances decomposed bears an infinitely small relation to the quantity present.

The actual rate varies with the quantity of hydrogen chlorate, in the first place *directly* as it is the substance decomposed, and in the second place with a small acceleration proportional to the quantity, the substance thus having a coefficient of acceleration independent of its being that undergoing decomposition. Thus

$$R = aQ(1+bQ),$$

where R is rate of decomposition, Q quantity. The variation with quantity of hydrogen chloride is not of so simple a nature. This acid would seem to have (1) an effect of the secondary order above mentioned (accelerative) on the decomposition of hydrogen chlorate alone; and in addition to this (2) an effect of both primary and secondary order as above on the decomposition of hydrogen chlorate *by* hydrogen chloride.

The addition of potassium chloride to the liquid has a small accelerative effect on the rate proportional to its quantity.

If a mixture of solution of potassium chlorate and hydrogen chloride is made (in molecular proportion between 1 : 2 and 1 : 12), complete double decomposition ensues. The hydrogen chlorate formed in presence of the remaining hydrogen chloride liberates oxidising

material as above, and the potassium chloride formed exercises its specific effect on this reaction.

The small quantity of potassium iodide added for the oxidising material to work upon is not concerned in the primary reaction. The secondary action upon it producing iodine is practically instantaneous, unless its quantity is below a certain minimum. Below this there is a retardation of the velocity apparent. The effect of increasing the amount of this substance beyond the minimum is apparently analogous to that of a similar increase of any neutral salt.

The velocity of decomposition is an exponential function of the temperature; as the latter increases in arithmetical progression, the former increases in geometrical progression. The velocity is about doubled for a rise of 5° C. in temperature. The ratio in the geometrical progression is not, however, absolutely constant, but varies a little with the actual temperature. Between 0° and 15° the velocity is a little more than doubled by a rise of 5°, between 20° and 30° a little less than doubled.

March 7, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates for election into the Society were read from the Chair, as follows:—

Aitken, John.
Anderson, William, M.I.C.E.
Armstrong, Robert Young, Lieut-Col., R.E.
Ballard, Edward, M.D.
Basset, Alfred Barnard, M.A.
Bosanquet, Robert Holford Macdowall, M.A.
Brown, Horace T., F.C.S.
Burbury, Samuel Hawkesley, M.A.
Buzzard, Thomas, M.D.
Cameron, Sir Charles Alexander, M.D.
Carnelley, Professor Thomas, D.Sc.
Clark, Latimer, C.E.
Conroy, Sir John, Bart., M.A.
Corfield, William Henry, M.D.
Cunningham, Professor Daniel John, M.D.
Cunningham, Professor David Douglas, M.B.
Dawson, George Mercer, D.Sc.
Dibdin, W. J., F.C.S.
Dickinson, William Howship, M.D.
Dreschfeld, Professor Julius, F.R.C.P.
Dresser, Henry Eales, F.L.S.
Eaton, Rev. Alfred Edwin, M.A.
Elgar, Professor Francis, LL.D.
Ewart, Professor J. Cossar, M.D.
Fleming, George, C.B.
Fletcher, Lazarus, M.A.
Frankland, Professor Percy Faraday, B.Sc.
Galloway, William.
Gilchrist, Percy C.
Gordon, James Edward Henry, B.A.
Hammond, James, M.A.
Harcourt, Leveson Francis Vernon, M.A.
Hemsley, William Botting, A.L.S.
Hinde, George Jennings, Ph.D.
Howorth, Henry Hoyle.
Hudson, Charles Thomas, M.A.
Hughes, Professor Thomas McKenny, M.A.
King, George.
Lansdell, Rev. Henry, D.D.
Lydekker, Richard, B.A.
MacMahon, Percy Alexander, Major, R.E.
Maitland, Major-General Eardley, C.B.
Martin, John Biddulph, M.A.
Miall, Professor Louis C.
Mond, Ludwig, F.C.S.
Ord, William Miller, M.D.
Palmer, Henry Spencer, Major-General, R.E.

Pedler, Professor Alexander, F.C.S.
Poulton, Edward B., M.A.
Roberts, Isaac, F.R.A.S.
Ross, James, M.D.
Sankey, Matthew Henry P. R., Capt., R.E.
Saunders, Howard, F.L.S.
Seebohm, Henry, F.L.S.
Sharp, David, M.B.
Shaw, William Napier, M.A.
Smith, Willoughby.
Sollas, Professor William Johnson, D.Sc.
Stebbing, Rev. Thomas Roscoe Rede, M.A.
Stevenson, Thomas, M.D.
Stewart, J. H. M. Shaw, Major-Gen., R.E.
Sutton, J. Bland, F.R.C.S.
Thin, George, M.D.
Thompson, Professor Silvanus Phillips, D.Sc.
Thomson, Professor John Millar, F.R.S.E.
Tidy, Professor Charles Meymott, M.B.
Todd, Charles, M.A.
Tomlinson, Herbert, B.A.
Weldon, Walter Frank Raphael, M.A.
Whitehead, Charles, F.L.S.
Yeo, Professor Gerald F., M.D.

The following Papers were read:—

I. "On the Composition of Water." By LORD RAYLEIGH, Sec. R.S. Received February 26, 1889.

During the past year I have continued the work described in a former communication on the relative densities of hydrogen and oxygen,* in the hope of being able to prepare lighter hydrogen than was then found possible. To this end various modifications have been made in the generating apparatus. Hydrogen has been prepared from potash in place of acid. In one set of experiments the gas was liberated by aluminium. In this case the generator consisted of a large closed tube sealed to the remainder of the apparatus; and the aluminium was attached to an iron armature so arranged that by means of an external electro-magnet it was possible to lower it into the potash, or to remove it therefrom. The liberated gas passed through tubes containing liquid potash,† corrosive sublimate, finely powdered solid potash, and, lastly, a long length of phosphoric anhydride. But the result was disappointing; for the hydrogen proved to be no lighter than that formerly obtained from sulphuric acid.

I have also tried to purify hydrogen yet further by absorption in palladium. In his recent important memoir,‡ "On the Combustion of weighed Quantities of Hydrogen and the Atomic Weight of Oxygen,"

* 'Roy. Soc. Proc.,' February, 1888 (vol. 43, p. 356).

† Of course this tube was superfluous in the present case, but it was more convenient to retain it.

‡ 'Amer. Chem. Journ.,' vol. 10, No. 4.

Mr. Keiser describes experiments from which it appears that palladium will not occlude nitrogen—a very probable impurity in even the most carefully prepared gas. My palladium was placed in a tube sealed, as a lateral attachment, to the middle of that containing the phosphoric anhydride; so that the hydrogen was submitted in a thorough manner to this reagent both before and after absorption by the palladium. Any impurity that might be rejected by the palladium was washed out of the tube by a current of hydrogen before the gas was collected for weighing. But as the result of even this treatment I have no improvement to report, the density of the gas being almost exactly as before.

Hitherto the observations have related merely to the densities of hydrogen and oxygen, giving the ratio 15·884, as formerly explained. To infer the composition of water by weight, this number had to be combined with that found by Mr. Scott as representing the ratio of volumes. The result was

$$\frac{2 \times 15{\cdot}884}{1{\cdot}9965} = 15{\cdot}914.$$

The experiments now to be described are an attempt at an entirely independent determination of the relative weights by actual combustion of weighed quantities of the two gases. It will be remembered that in Dumas's investigation the composition of water is inferred from the weights of the oxygen and of the water, the hydrogen being unweighed. In order to avoid the very unfavourable conditions of this method, recent workers have made it a point to weigh the hydrogen, whether in the gaseous state as in the experiments of Professor Cooke and my own, or occluded in palladium as in Mr. Keiser's practice. So long as the hydrogen is weighed, it is not very material whether the second weighing relate to the water or to the oxygen. The former is the case in the work of Cooke and Keiser, the latter in the preliminary experiments now to be reported.

Nothing could be simpler in principle than the method adopted. Globes of the same size as those employed for the density determinations are filled to atmospheric pressure with the two gases, and are then carefully weighed. By means of Sprengel pumps the gases are exhausted into a mixing chamber, sealed below with mercury, and thence by means of a third Sprengel are conducted into a eudiometer, also sealed below with mercury, where they are fired by electric sparks in the usual way. After sufficient quantities of the gases have been withdrawn, the taps of the globes are turned, the leading tubes and mixing chamber are cleared of all remaining gas, and, after a final explosion in the eudiometer, the nature and amount of the residual gas are determined. The quantities taken from the globes can be found from the weights before and after operations.

From the quantity of that gas which proved to be in excess, the calculated weight of the residue is subtracted. This gives the weight of the two gases which actually took part in the combustion.

In practice, the operation is more difficult than might be supposed from the above description. The efficient capacity of the eudiometer being necessarily somewhat limited, the gases must be fed in throughout in very nearly the equivalent proportions; otherwise there would soon be such an accumulation of residue that no further progress could be made. For this reason nothing could be done until the intermediate mixing chamber was provided. In starting a combustion, this vessel, originally full of mercury, was charged with equivalent quantities of the two gases. The oxygen was first admitted until the level of the mercury had dropped to a certain mark, and subsequently the hydrogen down to a second mark, whose position relatively to the first was determined by preliminary measurements of volume. The mixed gases might then be drawn off into the eudiometer until exhausted, after which the chamber might be recharged as before. But a good deal of time may be saved by replenishing the chamber from the globes simultaneously with the exhaustion into the eudiometer. In order to do this without losing the proper proportion, simple mercury manometers were provided for indicating the pressures of the gases at any time remaining in the globes. But even with this assistance close attention was necessary to obviate an accumulation of residual gas in the eudiometer, such as would endanger the success of the experiment, or, at least, entail tedious delay. To obtain a reasonable control, two sparking places were provided, of which the upper was situate nearly at the top of the eudiometer. This was employed at the close, and whenever in the course of the combustion the residual gas chanced to be much reduced in quantity; but, as a rule, the explosions were made from the lower sparking point. The most convenient state of things was attained when the tube contained excess of oxygen down to a point somewhat below the lower sparking wires. Under these circumstances, each bubble of explosive gas readily found its way to the sparks, and there was no tendency to a dangerous accumulation of mixed gas before an explosion took place. When the gas in excess was hydrogen, the manipulation was more difficult, on account of the greater density of the explosive gas retarding its travel to the necessary height.

In spite of all precautions several attempted determinations have failed from various causes, such as fracture of the eudiometer and others which it is not necessary here to particularise, leading to the loss of much labour. Five results only can at present be reported, and are as follows:—

December	24, 1888	....	15·93
January	3, 1889	....	15·98
"	21, "	....	15·98
February	2, "	....	15·93
"	13, "	....	15·92
		Mean..........	15·95

This number represents the atomic ratio of oxygen and hydrogen as deduced immediately from the weighings with allowance for the unburnt residue. It is subject to the correction for buoyancy rendered necessary by the shrinkage of the external volume of the globes when internally exhausted, as explained in my former communication.* In these experiments, the globe which contained the hydrogen was the same (14) as that employed for the density determinations. The necessary correction is thus four parts in a thousand, reducing the final number for the atomic weight of oxygen to

15·89,

somewhat lower than that which I formerly obtained (15·91) by the use of Mr. Scott's value of the volume ratio. It may be convenient to recall that the corresponding number obtained by Cooke and Richards (corrected for shrinkage) is 15·87, while that of Keiser is 15·95.

In the present incomplete state of the investigation, I do not wish to lay much stress upon the above number, more especially as the agreement of the several results is not so good as it should be. The principal source of error, of a non-chemical character, is in the estimation of the weight of the hydrogen. Although this part of the work cannot be conducted under quite such favourable conditions as in the case of a density determination, the error in the difference of the two weighings should not exceed 0·0002 gram. The whole weight of the hydrogen used is about 0·1 gram;† so that the error should not exceed three in the last figure of the final number. It is thus scarcely possible to explain the variations among the five numbers as due merely to errors of the weighings.

* The necessity of this correction was recognised at an early stage, and, if I remember rightly, was one of the reasons which led me to think that a redetermination of the density of hydrogen was desirable. In the meantime, however, the question was discussed by Agamennone ('Atti (Rendiconti) d. R. Accad. dei Lincei,' 1885), and some notice of his work reached me. When writing my paper last year I could not recall the circumstances; but since the matter has attracted attention I have made inquiry, and take this opportunity of pointing out that the credit of first publication is due to Agamennone.

† It was usual to take for combustion from two-thirds to three-fourths of the contents of the globe.

The following are the details of the determination of February 2, chosen at random :—

Before combustion .... $G_{14}+H+0{\cdot}2906 = G_{11}$ .... pointer 20·05
After " ... $G_{14}+H+0{\cdot}4006 = G_{11}$ .... pointer 20·31

$$\text{Hydrogen taken} = 0{\cdot}1100-0{\cdot}00005 = 0{\cdot}10995 \text{ gram.}$$

Before combustion .... $G_{13}+O = G_{11}+2{\cdot}237$ ... pointer 20·00
After " .... $G_{13}+O = G_{11}+1{\cdot}357$ ... pointer 19·3

$$\text{Oxygen taken} = 0{\cdot}8800+0{\cdot}0001 = 0{\cdot}8801 \text{ gram.}$$

At the close of operations the residue in the eudiometer was oxygen, occupying 7·8 c.c. This was at a total pressure of $29{\cdot}6-16{\cdot}2 = 13{\cdot}4$ inches of mercury. Subtracting 0·4 inch for the pressure of the water vapour, we get 13·0 as representing the oxygen pressure. The temperature was about 12° C. Thus, taking the weight of a cub. cent. of oxygen at 0° C. and under a pressure of 76·0 cm. of mercury to be 0·00143 gram, we get as the weight of the residual oxygen

$$0{\cdot}00143\,\frac{7{\cdot}8}{1+12\times 0{\cdot}00367}\,\frac{13{\cdot}0\times 2{\cdot}54}{76{\cdot}0} = 0{\cdot}0046 \text{ gram.}$$

The weight of oxygen burnt was, therefore, $0{\cdot}8801-0{\cdot}0046 = 0{\cdot}8755$ gram.

Finally, for the ratio of atomic weights,

$$\frac{\text{Oxygen}}{\frac{1}{2}\text{ Hydrogen}} = 15{\cdot}926.$$

In several cases the residual gas was subjected to analysis. Thus after the determination of February 2, the volume was reduced by additions of hydrogen to 1·2 c.c. On introduction of potash there was shrinkage to about 0·9, and, on addition of pyrogallic acid, to 0·1 or 0·2. These volumes of gas are here measured at a pressure of $\frac{1}{3}$ atmosphere, and are, therefore, to be divided by 3 if we wish to estimate the quantities of gas under standard conditions. The final residue of (say) 0·05 c.c. should be nitrogen, and, even if originally mixed with the hydrogen—the most unfavourable case—would involve an error of only $\frac{1}{8000}$ in the final result. The 0·1 c.c. of carbonic anhydride, if originally contained in the hydrogen, would be more important; but this is very improbable. If originally mixed with the oxygen, or due to leakage through india-rubber into the combustion apparatus, it would lead to no appreciable error.

The aggregate impurity of 0·15, here indicated, is tolerably satisfactory in comparison with the total quantity of gas dealt with—

2000 c.c. It is possible, however, that nitrogen might be oxidised, and thus not manifest itself under the above tests. In another experiment the water of combustion was examined for acidity, but without definite indications of nitric acid. The slight reddening observed appeared to be rather that due to carbonic acid, some of which, it must be remembered, would be dissolved in the water. These and other matters demand further attention.

The somewhat complicated glass blowing required for the combustion apparatus has all been done at home by my assistant, Mr. Gordon, on whom has also fallen most of the rather tedious work connected with the evacuation of globes and other apparatus, and with the preparation of the gases.

II. "On the Wave-length of the Principal Line in the Spectrum of the Aurora." By William Huggins, D.C.L., LL.D., F.R.S. Received February 19, 1889.

Notwithstanding the large number of determinations by different observers, since Ångström in 1867, of the wave-length of the principal (and frequently the only) line in the spectrum of the Aurora, this value has not yet been accepted as definitely fixed with the degree of accuracy which is required for a final inquiry into its chemical origin. The uncertainty within rather wide limits, which seems still to obtain, has arisen mainly from the circumstance that in nearly all cases the observations have been made with a small direct-vision spectroscope, and under conditions which do not admit of an accurate determination of the value sought for. About half the number of some twenty-four observers agree pretty well, but among the results given by the others the differences are very large in relation to the accuracy which is required, though they are not greater, perhaps, than was to be expected from the circumstances under which the observations were made.

I think it is very desirable, therefore, that I should put on record some observations of the spectrum of the Aurora which I made in the year 1874, but which up to the present time have remained unpublished. These observations were made with a powerful spectroscope, and under conditions which enabled me to determine the wave-length of the principal line within narrow limits of error. The spectroscope was made by Sir Howard Grubb on the automatic principle of his father, Mr. Thomas Grubb. It is furnished with two "Grubb" compound prisms; each has 5 square inches of base, and gives nearly twice the dispersion of a single prism of 60°, namely, about 9° 6′ from A to H.

The object-glasses of the collimator and telescope are 1·25 inch in

diameter. The definition is very good. Though the automatic arrangement works well, I always take the precaution to measure only small differences of position of the line to be determined from lines near it, the wave-lengths of which are known.

The observations were made on February 4, 1874. There was a brilliant Aurora, showing a whitish light; a direct-vision spectroscope resolved this light into a brilliant line in the yellow and a faint continuous spectrum.

The "Grubb" spectroscope was directed from the window of the observatory upon the brightest part of the Aurora. In the first instance, an estimation by eye was made of the position of the bright line by comparing it in the instrument with the spectrum of a spirit lamp. The bright line was seen to fall on the more refrangible side of the line for which Watts gives the wave-length 5582,* Ångström and Thalén 5583,† by from one-fifth to one-fourth of the distance of this line from the beginning of the band. If we take one-fourth, we have λ 5569·6; one-fifth gives λ 5572·3. The mean of these values gives for the

Aurora line λ 5570·9 .... (1).

The cross-wires of the spectroscope were then brought upon the line, and the reading 3476 showed the line to fall about midway between two strong lines in the spectrum of tin, λ 5564 and λ 5587 respectively, according to my measures.‡ The position of the cross was then compared directly with those lines in the spectrum of an induction spark taken between electrodes of tin. The further details of this comparison are not given in my note-book, but the result only, which placed the

Aurora line at λ 5571 .... (2).

Consulting my map of the chemical elements, I found that there was a line of tellurium very near this place, namely, at λ 5575, I therefore brought the spark from tellurium before the slit, when the cross appeared on the more refrangible side of the tellurium line. The measure of the distance of the cross from this line came out equal to λ 0003. The place given in my paper for this line of tellurium is 5575. Thalén gives for the same line 5574·1.§ If we take the mean of these values and deduct 0003, we get for

The line of the Aurora λ 5571·5 .... (3).

There are strong lines of iron very near this position in the

* 'Phil. Mag.,' vol. 41, 1871, p. 14.

† "Spectres des Métalloïdes," 'Nov. Act. Soc. Sci. Upsal.,' vol. 9, 1875 (p. 29).

‡ "Spectra of the Chemical Elements," 'Phil. Trans.,' 1864, p. 139.

§ 'Brit. Assoc. Rep.,' 1885, p. 292.

spectrum, and I made use of these also for a further determination of the place of the Aurora line. The cross, after having been placed upon the line of the Aurora, was confronted with these lines in the spectrum of iron.

The condensed account in my note-book does not give further particulars of this comparison, but states only that the place of the

Aurora line came out λ 5571·5 .... (4).

Summing up these determinations we have—

(1)	Eye-estimation	λ 5570·9
(2)	From tin	5571·0
(3)	From tellurium	5571·5
(4)	From iron	5571·5

From these values I think that we are justified in taking for the Aurora line, as a position very near the truth,

λ 5571 ± 0·5 .... (5).

Among the numerous determinations of other observers, those of Professor H. C. Vogel in 1872* seem to me to have great weight. A direct-vision spectroscope with a set of five prisms was used. The reduction of the readings of the micrometer into wave-lengths was based upon the repeated measures of 100 lines of the solar spectrum.

The screw had been thoroughly examined. After each observation of the Aurora line, readings were taken of the lines of sodium or of hydrogen. The observations extended over four nights. On three nights four separate readings were obtained; on the fourth night two only. Vogel gives as the mean result of the fourteen observations,

Aurora line λ 5571·3 ± 0·92 .... (6).

Perhaps I should state that I find, from a remark in my note-book, that at the time of my observations in 1874 I was not aware of Vogel's results, and I could not, therefore, have been biassed in any way by them.

The recent observations on the spectrum of the Aurora by Gyllenskiöld, at Cap Thordsen, in 1882, deserve special mention.† With a Hoffmann spectroscope, furnished with a scale, he obtained at Cap Thordsen in 1882 a mean result of λ 5568 ± 1·6; later, in 1884, at Upsala, with a Wrede spectroscope furnished with a micrometer screw, a mean value for the Aurora line, λ 5569 ± 6·2.‡ Gyllenskiöld

* 'Leipzig Math. Phys. Berichte,' vol. 22, p. 285.

† 'Observations faites au Cap Thordsen, Spitzberg, par l'Expédition Suédoise,' vol. 2, I:—Aurores Boréales, par Carlheim-Gyllenskiöld. Stockholm, 1886.

‡ *Ibid.*, p. 166.

discusses in detail nearly all the recorded observations of the spectrum of the Aurora from 1867 to 1882, and then brings them together in a table, with such probable errors as the original statements of the observers enabled him to assign to them. I think it is desirable to give that part of his list which contains the observations of the brightest line :—

1867.	Ångström	Upsal............	λ 5567 ± 1·0
1868.	Struve..........	Poulkowa........	5552 ± 14·9
	Lemström	Tromsœ........ ..	5659 ± 14·0
1869.	Peirce	États Unis........	5565 ± 10·8
1870.	Proctor		5595 ± 25·0
1871.	Smyth..........	Édimbourg.......	5579 ± 9·5
	Lindsay	Aberdeen	5680 ± 50·0
	Barker..........	New Haven	5594 ± 13·0
1872.	Vogel	Kiel	5571 ± 0·9
	Denza	Moncalieri........	5568 ± 11·9
	Donati..........	Florence	5569 ± 10·0
	Oettingen	Dorpat	5548 ± 30·0
	Respighi	Rome............	5574 ± 10·0
	Wijkander	Spitzberg	5572 ± 1·0
1873.	Backhouse	Sunderland	5660 ± 10·0
	Barker..........	New Haven	5569 ± 13·9
	Lemström	Enare	5569 ± 0·5
1874.	Backhouse	Sunderland	5570 ± 10·0
	Maclear	"Challenger"	5522 ± 37·1
1879.	Nordenskiold	Pitlekaïe.........	5563 ± 10·0
1880.	Copeland........	Dunecht	5572 ± 2·0
1882.	Gyllenskiöld	Cap Thordsen	5568 ± 1·6
1884.	"	Upsal............	5569 ± 6·2

Gyllenskiöld then calculates by the method of least squares the mean value of all the determinations, and finds the following result :—*

Mean value of the 23 observations, λ 5570·0 ± 0·88 ...... (7).

The recent measures by C. C. Krafft,† depart largely from Gyllenskiöld's mean value. Krafft found on

1882, November 2	λ 5595
" " 11	5586

and measures with the same instrument made by Schroeter on November 17th, gave λ 5587.

* *Ibid.*, p. 169

† 'Beobachtungs-Ergebnisse der Norwegischen Polarstation,' &c. A. S. Steen. Christiania, 1888.

Now, though Ångström's original value λ 5567 may not be quite accurate, his observation fixed a limit towards the red beyond which the Aurora line cannot lie. Angström says, "sa lumière était presque monochromatique, et consistait d'une seule raie brillante située à gauche" (on the more refrangible side) "du groupe connu des raies du calcium."* The position of the most refrangible line of this calcium-group is accurately known; according to†

Kirchhoff	λ 5580·9
Thalén	5580·9
Huggins	5581·0

It is certain, therefore, from Ångström's first observation in 1867 alone, that the Aurora line lies well on the more refrangible side of wave-length 5580. This limit towards the red was confirmed afterwards by Ångström himself; he says later that the yellow line falls almost midway between the second and third line of the shaded carbon group.‡ The positions of those lines of comparison are, according to Ångström and Thalén, λ 5538 and λ 5583.§

It follows that Krafft's values, λ 5586, λ 5587, and λ 5595, must be from some cause inaccurate. A possible explanation may be found in the small number of solar lines employed by Krafft for the reduction of the measures into wave-lengths. The curve was drawn through the six Fraunhofer lines B, C, *a*, D, E, and *b*. There was no control for the curve between D and E, and a very small deviation of the curve from its true position here would be sufficient to account for the position of less refrangibility of from λ 0016 to λ 0024, which his measures give for the Aurora line.

It should be stated that Krafft expresses regret that more attention could not be given to the spectroscopic observations. He says:—"Leider gestatteten die obligatorischen Beobachtungen nicht, den spectroscopischen Untersuchungen die gehörige Aufmerksamkeit angedeihen zu lassen. . . . . Ich glaubte ausserdem' diese Messungen um so mehr auslassen zu können, als der Platz der gewöhnlichen Nordlichtlinie oft und sehr genau bestimmt ist."

To sum up, we have the following values for the principal line of the Aurora:—

(6) 1872, Vogel	λ 5571·3 ± 0·92
(5) 1874, Huggins	5571·0 ± 0·5
(7) Gylleuskiöld's mean of 23 observers from 1867 to 1884	5570·0 ± 0·88

* 'Spectre Solaire,' Upsal, 1868, p. 42.
† 'Brit. Assoc. Rep.,' 1884, p. 372.
‡ 'Nature,' vol. 10, p. 211.
§ 'Acta Upsal.,' vol. 9, 1875 (p. 29).

These values agree closely, and fix within very narrow limits, the position in the spectrum, where we have to seek the chemical origin of the line.

Gyllenskiöld, from his observations of the changes which occur in the spectrum of the Aurora, comes to the conclusion that: "le spectre de l'Aurore boréale résulte de la superposition de plusieurs spectres différents," and that "la raie principale forme un de ces spectres élémentaires; elle apparait très souvent seule." A similar view was taken many years ago by Ångström* and by Vogel.†

[After consideration, I think that I ought to point out that Mr. Lockyer's recent statement‡ that:—"The characteristic line of the aurora is the remnant of the brightest manganese fluting at 558," is clearly inadmissible, considering the evidence we have of the position of this line.

In support of this statement Mr. Lockyer says:—"Ångström gave the wave-length of the line as 5567, and since then many observers have given the same wave-length for it, but probably without making independent determinations. Piazzi Smyth, however, gives it as 558, which agrees exactly with the bright edge of the manganese fluting. R. H. Proctor also gives the line as a little less refrangible than Ångström's determination. He says:—'My own measures give me a wave-length very slightly greater than those of Winlock and Ångström' ('Nature,' vol. 3, p. 468)."

By reference to Gyllenskiöld's table it will be seen that the probable errors of the determinations by Piazzi Smyth and Proctor, 5579 $\pm$ 9·5 and 5595 $\pm$ 25 0 respectively,§ are too large to entitle these measures to special weight.

Mr. Lockyer says further:—"Gyllenskiöld's measures with the Wrede spectroscope also give 5580 as the wave-length of the characteristic line. I feel justified, therefore, in disregarding the difference between the wave-length of the edge of the manganese fluting and the generally accepted wave-length of the aurora line."

Gyllenskiöld's single measure of 5580, on which Mr. Lockyer relies, differs widely from the values which Gyllenskiöld himself assigns to this line, namely, from observations at Cape Thordsen in 1882, λ 5568 $\pm$ 1·6, and from observations at Upsala in 1884, with the Wrede spectroscope, λ 5569 $\pm$ 6·2.

Speaking of Krafft's observations, Mr. Lockyer says:||—"The wave-

* 'Nature,' vol. 10, p. 210.

† 'Leipzig, Math. Phys. Berichte,' vol. 23, p. 298.

‡ 'Roy. Soc. Proc.,' vol. 45 (1889), p. 234.

§ Gyllenskiöld's statement of Proctor's value is based on 'Nature,' vol. 3, p. 347 and p. 68.

|| 'Roy. Soc. Proc.,' vol. 45 (1889), p. 241.

lengths obtained for the aurora line were 5595, 5586, and 5587. Unlike most observations, these place the aurora line on the less refrangible side of the manganese fluting. Hence, we have an additional reason for neglecting the difference between the wave-length of the brightest edge of the manganese fluting, and the commonly accepted wave-length of the aurora line, as given by Ångström. . . . . These observations are the latest which have been published, and were obviously made with a full knowledge of all previous work, so that their importance must be strongly insisted upon."

I have already pointed out that Krafft's measures were not made under circumstances which assured to them a high degree of accuracy; and Krafft's own words, which I have quoted, disclaim expressly any special attempt on his part to redetermine the position of the principal line with a higher degree of accuracy than the observers who preceded him.—March 4.]

III. "On the Cranial Nerves of Elasmobranch Fishes. Preliminary Communication." By J. C. EWART, M.D., Regius Professor of Natural History, University of Edinburgh. Communicated by Professor B. SANDERSON, F.R.S. Received February 22, 1889.

[Publication deferred.]

Presents, March 7, 1889.

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March 14, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Organisation of the Fossil Plants of the Coal-measures; Part XVI." By W. C. WILLIAMSON, LL.D., F.R.S., Professor of Botany in the Owens College, Manchester. Received March 5, 1889.

(Abstract.)

In this memoir the author first calls attention to detached observations made in his earlier memoirs relating to the manner in which a medullary axis is developed in the interior of each of the primary vascular bundles of the Carboniferous Lycopodiaceæ. He then traces the changes undergone during the development of a small branch-bundle in *Lepidodendron Harcourtii*. This is followed by a description of a small new species of Lepidodendron, which he named *L. mundum*, and in which the peculiar development of the medulla is clearly demonstrated.

In a second new species, named *Lepidodendron intermedium*, a peculiar and apparently early form of exogenous zone is shown to exist. When describing, in his previous memoir, Part XI (see Plate 49, fig. 11), the stem now designated *Lepidodendron fuliginosum*, he showed that, in it, we have an example of the most rudimentary and least perfectly developed form of an exogenous zylem yet seen amongst these Carboniferous Cryptogams. In this example, but a few radiating laminæ of vascular tissues make their appearance in the innermost cortex. In the *L. intermedium*, now described, though these few laminæ are represented by a continuous cylindrical zone of tracheids, and though the laminæ are arranged in radial order, they are still imbedded in a mass of cellular tissue, much in excess of what constitutes the medullary rays in the higher types of Lepidodendroid organisation.

A fourth new species of *Lepidodendron* is described under the name *L. Spenceri*, in young states of which no medulla is visible; but in its place a number of vertically elongated cells and imperfectly

lignified scalariform tracheids are seen, enclosed within an outer series of perfectly lignified ones. Here we have obviously an example of the centripetal development of a vascular bundle, reminding us of what is the normal mode of growth amongst the bundles of all recent Lycopods.

A fifth new species, *Lepidodendron parvulum*, is also described; after which the author points out the differences between the mode of development of the cellular medulla of these exogenous Cryptogams, and that of the representative organ amongst the Dicotyledonous Exogens. Amongst the ordinary Exogens the growing tip of a stem or branch is a mere aggregation of cells, which mass is soon separated into two zones, in addition to the formation of the epiderm, by the development within it of a ring of vascular bundles. The cells enclosed within this ring become the medulla or pith, and those external to it constitute the cortex. In this instance the cells about to form the medulla exist, prior to their becoming defined as a medulla by the first development of the vessels which enclose it, and which vessels will ultimately grow into a woody, or xylem, zone. Such a pith subsequently undergoes but a very limited enlargement. In most cases a time arrives when it grows less with age, and ultimately almost disappears; but in the Lepidodendra, though the tip of each growing stem was, in the first instance, also a cellular mass, what is designated an axial solid bundle of vessels was developed in the centre of the new growth almost at its very commencement. But it was only after this growth had made some progress, and the twig had become clothed externally with numerous leaves, that the first traces of a medulla began to appear in the centre of the bundle. It is thus clear that the medulla of these Carboniferous Lycopods is not genetically homologous with that of an ordinary exogenous flowering plant. But the stage of growth of the stem at which this medulla first appeared has differed remarkably in various species of *Lepidodendron*, a remark equally applicable to the first formation in them of a true exogenous zone.

The axial vascular medullary bundle expanded into a hollow cylinder under the internal pressure of the growing medulla, which latter not only attained to considerable dimensions, but was a persistent organ. This ring enclosing the medulla, supplied the vascular bundles going to leaves and branches. The author demonstrates that the branches are supplied with such bundles in two ways. When the growing stem divides dichotomously, which it does as amongst living Lycopods, the medullary vascular cylinder splits into two equal halves. But, besides this mode, the author shows that very frequently comparatively small segments are cut completely out of the vascular cylinder, in which a gap is thus left where the bark and the medulla meet. The angular segment thus detached develops,

as it ascends through the bark, into a solid cylindrical bundle, in which, in time, a medulla forms as before. The author is inclined to believe that all these latter forms of bundles only supply short abortive lateral branches, which most probably supported Lepidostrobous fruits.

II. "A Method of examining Rate of Chemical Change in Aqueous Solutions." By G. Gore, F.R.S. Received January 11, 1889.

(Abstract.)

This research supplies an outline of a method of examining chemical change, based upon the application of the "voltaic balance" to measuring the relative amounts of voltaic energy of electrolytes (see 'Roy. Soc. Proc.,' vol. 44, pp. 151, 294), and the rate of chemical change is indicated by alterations in amount of such energy.

The author gives an example of two liquids, viz., a solution of equivalent proportions of potassic iodide and chlorine, and one of chloride of potassium and iodine, which, although having the same ultimate chemical composition, are greatly different (viz., as 1·0 to 31·76) in voltaic energy, and in a moderate degree different in colour. The latter of these is a nearly stable liquid, and does not readily alter in chemical composition at 13° C., whilst the former is extremely unstable, continually losing voltaic energy, and becoming darker in colour at that temperature, until it nearly acquires the chemical composition and properties of the other mixture.

From the results obtained it is concluded—1st, that the aqueous solution of equivalent proportions of potassic iodide and chlorine decomposes spontaneously at 12° C., with gradual formation of potassic chloride and liberation of iodine; 2nd, that the change of chemical composition is attended by considerable loss of voltaic energy; 3rd, that more than six days are necessary to effect the complete chemical change at that temperature; and, 4th, that the rate of chemical change is much greater at the commencement of the action than towards its termination. Further, that the solution of potassic chloride and iodine increases slightly in energy during the mixing.

The influence of dilution, time, temperature, light, agitation, and mode of mixing upon the chemical change was examined. It was found that the degree of dilution of the constituent liquids of the potassic iodide and chlorine solution *during the act of mixing* largely affected the amount of chemical change which occurred during mixture, but with the solution of potassic chloride and iodine the strength of the liquids had no such effect. The effect of dilution appears to be

related to the degree of mobility and diffusibility of the particles, and is largely modified by the degree of stability of the mixture.

Temperature had great effect upon the solution of potassic iodide and chlorine. Heating the liquid to about 100° C. during two minutes was attended by great loss of voltaic energy, considerable increase of colour, and about 99·8 per cent. of the mixture was changed into potassic chloride and free iodine; the amount of change was as great as that which took place during 18 days at 12° C. Similarly heating the solution of potassic chloride and iodine had but little effect; it, however, slightly increased its voltaic energy and decreased its colour, and so far changed it into the other mixture (?).

Exposing the solution of potassic iodide and chlorine to diffused daylight during 18 dull winter days at about 12° C. did not appear to greatly alter the rate of chemical change, as shown by alterations of colour and of voltaic energy. Light somewhat retarded the action.

Strong agitation during one minute of the freshly made solution appeared to slightly increase the amount of chemical change which occurred during mixture.

In making this solution the amount of chemical change which took place during mixing was about 5 per cent. more if the chlorine solution was added to the solution of iodide than if the order of addition was reversed.

The results of the experiments show that the solution of potassic iodide and chlorine was very unstable, highly sensitive to rise of temperature, had a great tendency to lose its voltaic energy, to change its chemical composition, and approach that of the other mixture; that the solution of the latter was very much more stable, and much less sensitive to heat, but had a feeble tendency to absorb energy, to change its chemical composition, and approach that of the solution of potassic iodide and chlorine. The effect, therefore, of heating both liquids was to produce two portions possessing similar chemical composition and properties, but much more nearly resembling the chloride than the iodide mixture, and consisting of about 0·23 part of potassic iodide, 74·49 of potassic chloride, 126·8 of iodine, and 0·0497 of chlorine.

The collective results show that the "voltaic balance" method may be used to detect changes of chemical composition of aqueous solutions, and to measure the rate of such change going on in them. Although the method as described does not give the amount of change which occurs during the mixing of the liquids, it gives the subsequent amounts of change with a reasonable degree of accuracy. Its great advantage over the colorimetric method is that it is equally applicable to colourless liquids; it is much more sensitive and exact than either the colorimetric or the thermochemical method; and it is quick and easy of performance. It is at present being used to detect and measure

chemical changes produced by light in aqueous solutions. The degree of freedom of an aqueous solution of chlorine from hydrochloric acid and of iodine from hydriodic acid was determined much more readily by means of the "voltaic balance" method than by ordinary chemical analysis.

III. "Relative Amounts of Voltaic Energy of dissolved Chemical Compounds." By G. GORE, F.R.S. Received January 16, 1889.

(Abstract.)

In this investigation the author has measured, by means of the "voltaic balance," the amounts of relative voltaic energy or of chemical affinity for zinc, of nearly 250 aqueous solutions of dissolved chemical compounds, at ordinary atmospheric temperatures. The substances include compounds of elements with elements; elements with monobasic, bibasic, and tribasic acids; acids of all these classes with each other; elements with monobasic, bibasic, tribasic, and tetrabasic salts; monobasic, bibasic, and tribasic acids with all these classes of salts; and all these classes of salts with each other in great variety. The method employed has been already described (see 'Roy. Soc. Proc.,' vol. 44, pp. 181, 294), and he offers the results thus obtained as additional evidence in support of the conclusion, that "*every electrolytic substance or mixture when dissolved in water unites chemically in definite proportions by weight with every other such dissolved body*, provided no separation of substance occurs;" and that "there may probably be discovered thousands of such compounds, which only exist whilst in aqueous solution, and are decomposed on evaporating or crystallising their solutions." The present research has shown the existence of nearly 250.

The formulæ of the compounds, together with the amounts of energy, are arranged in the form of a table as a volta tension series of electrolytes, commencing with I+Cl, which gives a plus number of +11,686,507, and ending with $2(H_3N+KHO)+(K_2CO_3+Na_2SO_3)$, which gives a minus one of −959,817. The whole of the formulæ agree with the ordinary chemical equivalents of the substances.

IV. "Note on the Free Vibrations of an infinitely long Cylindrical Shell." By LORD RAYLEIGH, Sec. R.S. Received February 26, 1889.

In a recent memoir* Mr. Love has considered this question among others; but he has not discussed his result [equation (95)], except in its application to a rather special case involving the existence of a free edge. When the cylinder is regarded as infinitely long, the problem is naturally of a simpler character; and I have thought that it might be worth while to express more fully the frequency equation, as applicable to all vibrations, independent of the thickness of the shell, which are periodic with respect both to the length and the circumference of the cylinder.

In order to prevent misunderstanding, it may be well to premise that the vibrations, whose frequency is to be determined, do not include the gravest of which a thin shell is capable. If the middle surface be simply bent, the potential energy of deformation is of a higher order of magnitude than in the contrary case, and according to the present method of treatment the frequency of vibration will appear to be zero. It is known, however, that the only possible modes of bending of a cylindrical shell are such as are not periodic along the length, or rather have the wave-length in this direction infinitely long.† When the middle surface is stretched, as well as bent, the potential energy of bending may be neglected, except in certain very special cases.

Taking cylindrical co-ordinates (r, ϕ, z), and denoting the displacements parallel to z, ϕ, r by u, v, w respectively, we have for the principal elongations and shear at any point (a, ϕ, z)‡—

$$\sigma_1 = \frac{du}{dz}, \qquad \sigma_2 = \frac{w}{a}+\frac{1}{a}\frac{dv}{d\phi}, \qquad \varpi = \frac{1}{a}\frac{du}{d\phi}+\frac{dv}{dz} \quad \ldots\ldots (1);$$

and the energy per unit of area is expressed by

$$2nh\left\{\sigma_1^2+\sigma_2^2+\tfrac{1}{2}\varpi^2+\frac{m-n}{m+n}(\sigma_1+\sigma_2)^2\right\} \quad \ldots\ldots\ldots (2),$$

where $2h$ denotes the thickness of the shell, and m, n are the elastic constants of Thomson and Tait's notation.

* "On the small Free Vibrations and Deformation of a thin Elastic Shell," 'Phil. Trans.,' A, vol. 179 (1888), p. 491.

† "On the Bending and Vibration of thin Elastic Shells, especially of Cylindrical Form," 'Roy. Soc. Proc.,' *supra*, p. 105.

‡ See a paper on the Infinitesimal Bending of Surfaces of Revolution ('London Math. Soc. Proc.,' vol. 13, p. 4, Nov. 1881), and those already cited.

The functions u, v, w are to be assumed proportional to the sines, or cosines, of μz and $s\phi$. These may be combined in various ways, but a sufficient example is

$$u = \mathrm{U}\cos s\phi\cos\mu z,\quad v = \mathrm{V}\sin s\phi\sin\mu z,\quad w = \mathrm{W}\cos s\phi\sin\mu z \;\ldots\ldots (3);$$

so that

$$\sigma_1 = -\mu\mathrm{U}\cos s\phi\sin\mu z \;\ldots\ldots\ldots\ldots (4),$$

$$\sigma_2 = (\mathrm{W}+s\mathrm{V})\cos s\phi\sin\mu z \;\ldots\ldots\ldots\ldots (5),$$

$$\varpi = (-s\mathrm{U}+\mu\mathrm{V})\sin s\phi\cos\mu z \;\ldots\ldots\ldots\ldots (6),$$

unity being written for convenience in place of a. The energy per unit area is thus

$$2nh\left[\cos^2 s\phi\sin^2\mu z\left\{\mu^2\mathrm{U}^2+(\mathrm{W}+s\mathrm{V})^2+\frac{m-n}{m+n}(\mathrm{W}+s\mathrm{V}-\mu\mathrm{U})^2\right\}\right.$$
$$\left.+\sin^2 s\phi\cos^2\mu z\,(-s\mathrm{U}+\mu\mathrm{V})^2\right]\ldots\ldots (7).$$

Again, the kinetic energy per unit area is, if ρ be the volume density,

$$\rho h\left[\left(\frac{d\mathrm{U}}{dt}\right)^2\cos^2 s\phi\cos^2\mu z+\left(\frac{d\mathrm{V}}{dt}\right)^2\sin^2 s\phi\sin^2\mu z+\left(\frac{d\mathrm{W}}{dt}\right)^2\cos^2 s\phi\sin^2\mu z\right]$$
$$\ldots\ldots (8).$$

In the integration of these expressions with respect to ϕ and z, the mean value of each $\sin^2$ or $\cos^2$ is $\frac{1}{2}$.* We may then apply Lagrange's method. If the type of vibration be $\cos pt$, and $p^2\rho/n = k^2$, the resulting equations may be written

$$\{2(\mathrm{M}+1)\mu^2+s^2-k^2\}\mathrm{U}-(2\mathrm{M}+1)\mu s\mathrm{V}-2\mathrm{M}\mu\mathrm{W} = 0\ldots\ldots (9),$$

$$-(2\mathrm{M}+1)\mu s\mathrm{U}+\{\mu^2+2(\mathrm{M}+1)s^2-k^2\}\mathrm{V}+2(\mathrm{M}+1)s\mathrm{W} = 0\ldots (10),$$

$$-2\mathrm{M}\mu\mathrm{U}+2(\mathrm{M}+1)s\mathrm{V}+\{2(\mathrm{M}+1)-k^2\}\mathrm{W} = 0\ldots (11),$$

where

$$\mathrm{M} = \frac{m-n}{m+n}\;\ldots\ldots\ldots\ldots (12).$$

The frequency equation is that expressing the evanescence of the determinant of this triad of equations.

We will consider for a moment the simple case which arises when $\mu = 0$, that is, when the displacements are independent of z. The three equations reduce to

* In the physical problem the range of integration for ϕ is from 0 to 2π; but mathematically we are not confined to one revolution. We may conceive the shell to consist of several superposed convolutions, and then s is not limited to be a whole number.

$$(s^2-k^2)\mathrm{U} = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (13),$$

$$\{2(\mathrm{M}+1)s^2-k^2\}\mathrm{V}+2(\mathrm{M}+1)s\mathrm{W} = 0 \quad \ldots\ldots\ldots \quad (14),$$

$$2(\mathrm{M}+1)s\mathrm{V}+\{2(\mathrm{M}+1)-k^2\}\mathrm{W} = 0 \quad \ldots\ldots\ldots \quad (15);$$

and they may be satisfied in two ways. First let $\mathrm{V} = \mathrm{W} = 0$; then U may be finite, provided

$$s^2-k^2 = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (16).$$

The corresponding type for U is

$$\mathrm{U} = \cos s\phi \cos pt \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (17),$$

where $$p^2 = \frac{ns^2}{\rho a^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (18),$$

a being restored, as can be done at any moment by consideration of dimensions. In this motion the material is sheared without extension, every generating line of the cylinder moving along its own length. The frequency depends upon the circumferential wave-length, and not upon the curvature of the cylinder.

The second kind of vibrations are those in which $\mathrm{U} = 0$, so that the motion is strictly in two dimensions. The elimination of the ratio V/W from (14), (15) gives

$$k^2\{k^2-2(\mathrm{M}+1)(1+s^2)\} = 0 \quad \ldots\ldots\ldots\ldots \quad (19),$$

as the frequency equation. The first root is $k^2 = 0$, indicating infinitely slow motion. These are the flexural vibrations already referred to, and the corresponding relation between V and W is by (14)

$$s\mathrm{V}+\mathrm{W} = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (20),$$

giving by (4), (5), (6),

$$\sigma_1 = \sigma_2 = \varpi = 0.$$

The other root of (19) gives on restoration of a,

$$k^2a^2 = \frac{4m}{m+n}(1+s^2) \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (21),$$

or $$p^2 = \frac{4mn}{m+n}\,\frac{1+s^2}{a^2\rho} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (22);$$

while the relation between V and W is

$$-\mathrm{V}+s\mathrm{W} = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (23).$$

It will be observed that when s is very large, the flexural vibrations tend to become exclusively normal, and the extensional vibrations to become exclusively tangential, as might have been expected from the theory of plane plates.

Returning now to the general case, the determinant of (9), (10), (11) gives on reduction

$$[k^2-\mu^2-s^2]\{k^2[k^2-2(M+1)(\mu^2+s^2+1)]+4(2M+1)\mu^2\}$$
$$+4(2M+1)\mu^2s^2 = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (24).$$

If $\mu = 0$, we have the three solutions already considered,

$$k^2 = 0, \qquad k^2 = s^2, \qquad k^2 = 2(M+1)(s^2+1).$$

If $s = 0$, that is, if the deformation be symmetrical about the axis, we have

$$k^2 = \mu^2, \quad \text{or} \quad k^2[k^2-2(M+1)(\mu^2+1)]+4(2M+1)\mu^2 = 0 \quad \ldots \quad (25).$$

Corresponding to the first root we have $U = 0$, $W = 0$, as is readily proved on reference to the original equations with $s = 0$. The vibrations are the purely torsional ones represented by

$$v = \sin \mu z \cos pt \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (26),$$

where

$$p^2 = \frac{n\mu^2}{\rho} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (27).$$

The frequency depends upon the wave-length parallel to the axis, and not upon the radius of the cylinder.

The remaining roots of (25) correspond to motions for which $V = 0$, or which take place in planes through the axis. The general character of these vibrations may be illustrated by the case where μ is small, or the wave-length a large multiple of the radius of the cylinder. We find approximately from the quadratic (on restoration of a)

$$\frac{k^2a^2}{M+1} = 2+\frac{2M^2\mu^2a^2}{(M+1)^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (28),$$

or

$$k^2 = \frac{2(2M+1)\mu^2}{(M+1)} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (29).$$

The vibrations of (28) are nearly purely radial. If we suppose that μ vanishes, we fall back upon

$$k^2a^2 = 2(M+1),$$

or $$p^2 = \frac{4mn}{m+n}\frac{1}{a^2\rho} \quad\dots\dots\dots\dots\dots\dots (30),*$$

as may be seen from (22), by putting $s = 0$.

On the other hand, the vibrations of (29) are nearly purely axial. In terms of m and n,

$$p^2 = \frac{n\mu^2}{\rho}\frac{3m-n}{m}, \quad\dots\dots\dots\dots\dots\dots (31).$$

Now, if q denote Young's modulus,

$$q = \frac{n(3m-n)}{m} \quad\dots\dots\dots\dots\dots\dots (32);$$

so that $$p^2 = \frac{q\mu^2}{\rho}\dots\dots\dots\dots\dots\dots\dots\dots (33).$$

This is the ordinary formula for the longitudinal vibrations of a rod, the fact that the section is here a thin annulus not influencing the result to this order of approximation.

Another extreme case worthy of notice occurs when s is very great. Equation (24) then reduces to

$$k^2[k^2-\mu^2-s^2][k^2-2(\mathrm{M}+1)(\mu^2+s^2)] = 0 \quad\dots\dots (34);$$

so that k^2 becomes a function of μ and s only through (μ^2+s^2), as might have been expected from the theory of plane plates. The first root relates to flexural vibrations; the second to vibrations of shearing, as in (18); the third to vibrations involving extension of the middle surface, analogous to those in (22).

It is scarcely necessary to add, in conclusion, that the most general deformation of the middle surface can be expressed by means of a series of such as are periodic with respect to s and ϕ, so that the problem considered is really the most general small motion of an infinite cylindrical shell.

[Another particular case worth notice arises when $s = 1$, so that (24) assumes the form

$$k^2(k^2-\mu^2-1)[k^2-2(\mathrm{M}+1)(\mu^2+2)]$$
$$+4\mu^2(k^2-\mu^2)(2\mathrm{M}+1) = 0 \quad\dots\dots\dots (35).$$

As we have already seen, if μ be zero, one of the values of k^2 vanishes. If μ be small, the corresponding value of k^2 is of the order μ^4. Equation (35) gives in this case

$$k^2 \doteq \frac{2\mathrm{M}+1}{\mathrm{M}+1}\mu^4 \quad\dots\dots\dots\dots\dots\dots (36);$$

* This equation is given, in a slightly different form, by Love (*loc. cit.*, p. 523).

March 21, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Velocity of Transmission through Sea-water of Disturbances of large Amplitude caused by Explosions." By RICHARD THRELFALL, M.A., Professor of Physics, and JOHN FREDERICK ADAIR, M.A., Demonstrator of Physics, University of Sydney. Communicated by Professor J. J. THOMSON, F.R.S. Received March 14, 1889.

(Abstract.)

This paper contains an account of a large number of experiments made with the object of determining the velocity of waves of compression caused by explosions under water.

The method adopted depended on the use of a certain "gauge" devised for the occasion, whereby the arrival of the disturbance at a given point was transmitted to a chronograph.

The disturbances themselves were caused by submarine explosions of dynamite and guncotton in quantities varying from nine ounces to four pounds.

The distance over which the velocity was measured was about 200 yards.

The water was that of the Pacific Ocean in the harbour of Port Jackson, N.S. Wales.

The chronograph was of the falling pendulum description, and fired the charge automatically.

The absolute time was obtained by comparing the chronograph tuning fork with an astronomical clock.

The distance was obtained in terms of the standard yard of N.S. Wales by means of trigonometrical survey. The chief results for the range quoted are as follows:—

Class.	Description of explosive.	Number of experiments (each experiment involving two explosions and time measurements).	Velocity found in metres per second.	Temperature C.	Velocity of sound calculated in metres per second.	Excess of velocity as compared with velocity of sound.
A	9 oz. dry guncotton.	11	1732±22	17°·8	1523	per cent. 13·75
B	10 oz. No. 1 dynamite	24	1775±27	14·5	1508	17·7
C	18 oz. dry guncotton.	5	1942±8	18·3	1525	27·3
D	64 oz. dry guncotton.	3	2013±35	19·0	1528	31·7

The chief portion of the paper is occupied by a description of the details of the precautions taken to make the measurements as accurate as possible.

II. "An Experimental Investigation of the Circumstances under which a Change of the Velocity in the Propagation of the Ignition of an Explosive Gaseous Mixture takes place in closed and open Vessels. Part I. Chronographic Measurements." By FREDERICK J. SMITH, M.A., Millard Lecturer, Exptl. Mech., Trin. Coll., Oxford. Communicated by A. G. VERNON HARCOURT, F.R.S. Received March 12, 1889.

(Abstract.)

The subject of the paper of which this is an abstract, is the determination of the rate of change of the velocity of the propagation of an explosion in gaseous explosive mixtures.

It has been noticed by several investigators, viz., MM. Berthelot and Vielle, MM. Mallard and Le Chatelier, and Professor H. B. Dixon, F.R.S., that explosive gaseous mixtures after ignition do not reach their maximum velocity of propagation at once, but that a certain maximum velocity is attained soon after initial ignition.

In order to investigate this interesting period, which may be called the acceleration period of an explosion, chronographic measurements of a peculiar nature were found necessary.

It was at once evident that but little advance in this branch of the subject of explosions could be made unless exceedingly minute periods of time could be measured with certainty.

It was not possible to work with the pendulum chronograph (good as this instrument is for other branches of research), as its length of traverse is too limited, and the difficulty of subdividing tuning fork traces is found to be very great, since the velocity of the pendulum varies from zero up to a maximum during its swing; this being so, a new form of chronograph has been devised to meet as far as possible all the requirements of the case, by means of the instrument. The following results have been obtained:—

1. The $\frac{1}{30000}$th second can be measured with ease, and periods of time differing from $\frac{1}{10}$th second to $\frac{1}{10000}$th second can be recorded on the same moving surface.

2. The surface which receives the record moves at a velocity which is practically constant during the traverse of 50 cm.; also its velocity can be varied between wide limits.

3. A large number of time records can be made side by side, all records being made in straight lines.

4. Fractions of recorded vibrations of a fork can be subdivided by means of a micrometer microscope. This is not the case with vibrations recorded on a surface attached to a pendulum, where the velocity varies from zero up to a maximum at the middle of the swing.

The electromagnetic styli, by means of which events are marked, are so constructed that their period of "latency" is almost absolutely constant, and their electromagnets are so wound that no sparking takes place on breaking the circuit.

A moving surface is carried on a carriage, which is propelled by means of a falling weight, which after a certain velocity has been attained is removed, the surface then moves with a velocity which is found to be practically constant, for the limits between which a time record is made.

The chronograph is used in conjunction with a steel tube in which the explosions take place. At even distances along the axis of the tube, conducting bridges, eight to ten in number, of Dutch metal insulated from the tube, are placed; each bridge is connected electrically with a recording stylus, so that as each bridge is broken by the explosion, a mark is made on the surface of the chronograph; these markings when duly interpreted provide data for constructing a curve, which indicates the rate at which the velocity of the explosion is changing during its propagation.

The rest of the paper treats of the methods by means of which the errors due to the use of electromagnets in chronographic work have been dealt with and reduced as far as possible.

III. "On an Effect of Light upon Magnetism." By SHELFORD BIDWELL, M.A., F.R.S. Received March 11, 1889.

Several experimenters in the early part of the present century tried to magnetise iron and steel by the action of light,* but I do not know of any recent attempts in this direction, and of late years the thing has been generally regarded as impossible. Under ordinary circumstances there can be little doubt that this is the case, but, if a certain condition is fulfilled, we might, I think, expect to find some evidence of the action of radiation upon the magnetism of iron.

The condition is that the susceptibility of the bar AB to be operated upon shall be greater (or less) for a magnetic force in the direction AB than for an equal one in the direction BA. This paper contains a short preliminary account of a series of experiments which have been undertaken with iron bars having this property. Much yet remains to be done, which will require a considerable amount of time, and for which special apparatus must be constructed. In the meantime, the results already obtained appear to possess sufficient interest to justify their publication.

The method of preparing the bars is as follows: A piece of soft iron rod, which may conveniently be 10 or 12 cm. long and from 0·5 to 1 cm. in diameter, is raised to a bright yellow heat and slowly cooled. When cold, it is placed inside a solenoid, through which is passed a battery current of sufficient strength to produce a field of about 350 or 400 C.G.S. units. The iron when removed from the coil is found to be permanently magnetised, and its north pole is marked for the sake of distinction with red sealing-wax varnish. The bar is then supported horizontally and in an east and west direction behind a small reflecting magnetometer, and over it is slipped a coil, which is shunted with a rheostat, the resistance of which can be gradually increased from 0 to 26 ohms. The coil can be connected by a key with a single battery cell, which is so arranged as to produce a demagnetising force inside the coil. The resistance of the rheostat is slowly raised, so that more and more current passes through the coil, the battery key being alternately lifted and depressed until the magnetometer indicates that the iron bar as a whole is perfectly demagnetised. The strength of the demagnetising force required varies according to circumstances: it is generally about one-thirtieth or one-twenty-fifth of the original magnetising force.

After this treatment the iron rod does not differ, so far as ordinary tests will show, from one which has never been submitted to mag-

* Chrystal, 'Encycl. Britann.,' vol. 15, p. 274, mentions the following Morichini, Mrs. Somerville, Christie, Riess, and Moser.

netic influences. Nevertheless, as is well known, it possesses certain properties which distinguish it from a piece of really virgin iron. In the first place, the magnetisation induced by a force acting in such a direction as to make the marked end a north pole, is greater than that caused by an equal force in an opposite direction. Again, if such a bar be held horizontally east and west (to avoid terrestrial influences), and tapped with a mallet, the marked end at once becomes a north pole. A similar effect follows if the rod be warmed in the flame of a spirit-lamp. Lastly, if it be placed inside a coil and subjected to the action of a series of rather feeble magnetic forces, of equal strength but alternating in direction, the marked end will generally become a north pole, even though the last of the alternate forces may have tended to induce the opposite polarity.

A rod treated as above described appears to be remarkably sensitive to the action of light. When such a rod is placed behind the magnetometer, and illuminated by an oxyhydrogen lamp about 70 cm. distant, there occurs an immediate deflection of from 10 to 200 scale divisions,* the magnitude of the effect varying in different specimens of iron. As the action of the light is continued, the deflection slowly increases. When the light is shut off, the magnetometer instantly goes back over a range equal to that of the first sudden deflection, then continues to move slowly in the backward direction towards zero.

The first quick movement I believe may be due to the direct action of radiation, and the subsequent slow movement to the gradually rising temperature of the bar. With a thick rod (1 cm. in diameter) the slow movement is barely perceptible, extending over only one or two scale divisions in the course of a minute, the spot of light becoming almost stationary after the first sudden jump. With a thin rod the sudden effect is generally smaller, while the slow after-effect is greater and may continue until the spot of light passes off the scale.

As a general rule the magnetic effect is such as to render the marked end of the rod a north pole: occasionally, however, it becomes a south pole, but in such cases I have always found that the polarity is comparatively feeble. It may even happen† that the marked end becomes north when certain portions of the rod are illuminated, and south when the light acts upon other portions. This is probably due to irregular annealing and a consequent local reversal of the direction of maximum susceptibility: it indicates that the light effect is local, and is confined to the illuminated surface. In one remarkable specimen, which happens not to have been annealed at all, the sudden effect and the slow effect are in opposite directions. When the light

* The magnetometer mirror was 1 metre distant from the scale and each division = 0·64 mm. ($\frac{1}{40}$ inch).

† This has been observed in two specimens.

is turned upon this rod, there is at first a sudden deflection of twenty magnetometer-scale-divisions to the left, the spot afterwards moving slowly and steadily towards the right. When the light is shut off there occurs at once a jump of twenty divisions further towards the right before the spot begins to move back in the zero direction.

Some attempts have been made to repeat the experiments with light polarised by means of a Nicol's prism; but, either because the largest prism at my disposal was too small (its aperture being barely 2 cm.), or because too much of the radiant energy was absorbed by the spar, I failed to get any magnetic effects whatever with the prism in either position.

[Professor Silvanus Thompson has quite recently been kind enough to lend me a very large and excellent Nicol's prism. From a few experiments already made with this instrument it appears that the action of the light is quite independent of the plane of polarisation.—March 16.]

There can be no doubt whatever of the reality of the effects here described: they are perfectly distinct, and are at any time reproducible with certainty. The only question is how much of them is primarily caused by the action of light, and how much by mere incidental change of temperature. But taking all the circumstances into consideration, I think the evidence is in favour of the conclusion that the *instantaneous* magnetic change, which occurs when a prepared iron bar is illuminated, is purely and directly an effect of radiation.

IV. "Recalescence of Iron." By J. HOPKINSON, F.R.S. Received March 7, 1889.

Professor Barrett has observed that if an iron wire be heated to a bright redness and then be allowed to cool, that this cooling does not go on continuously, but that after the wire has sunk to a very dull red it suddenly becomes brighter, and then continues to cool down. He surmised that the temperature at which this occurs is the temperature at which the iron ceases to be magnetisable. In repeating Professor Barrett's experiments, I found no difficulty in obtaining the phenomenon with hard steel wire, but I failed to observe it in the case of soft iron wire, or in the case of manganese steel wire. It appeared to be of interest to determine the actual temperature at which the phenomenon occurred, and also the amount of heat which was liberated. Although other explanations of the phenomenon have been offered, there can never, I think, have been much doubt that it was due to the liberation of heat owing to some change in the material, and not due to any change in the conductivity or emissive power. My method of experiment was exceedingly simple. I took a

cylinder of hard steel, 6·3 cm. long and 5·1 cm. diameter, cut a groove in it, and wrapped in the groove a copper wire insulated with asbestos. The cylinder was wrapped in a large number of coverings of asbestos paper to retard its cooling, the whole was then heated to a bright redness in a gas furnace, was taken from the furnace and allowed to cool in the open air, the resistance of the copper wire being from time to time observed. The result is plotted in the accompanying curve, in which the ordinates are the logarithms of the increments of resistance above the resistance at the temperature of the room, and the abscissæ are the times. If the specific heat of the material were constant, and the rate of loss of heat were proportional to the excess of temperature, the curve would be a straight line. It will be observed that below a certain point this is very nearly the case, but that there is a remarkable wave in the curve. The temperature was observed to be falling rapidly, then to be suddenly retarded, next to increase, then again to fall. The temperature reached in the first descent was 680° C. The temperature to which the iron subsequently ascends is 712° C. The temperature at which another sample of hard steel ceased to be magnetic, determined in the same way by the resistance of a copper coil, was found to be 690° C. This shows that within the limits of errors of observation the temperature of recalescence is that at which the material ceases to be magnetic. This curve gives the material for determining the

quantity of heat liberated. The dotted lines in the curve show the continuation of the first and second parts of the curve, the horizontal distance between these approximately represents the time during which the material was giving out heat without fall of temperature. After the bend in the curve the temperature is falling at the rate of 0·21° C. per second. The distance between the two curves is 810 seconds. It follows that the heat liberated in recalescence of this sample is 173 times the heat liberated when the iron falls in temperature 1° C. With the same sample I have also observed an ascending curve of temperature. There is in this case no reduction of temperature at the point of recalescence, but there is a very substantial reduction in the rate at which the temperature rises.

V. "Electrical Resistance of Iron at a High Temperature." By J. Hopkinson. Received March 14, 1889.

Auerbach, Callendar, and I think also Tait, have observed that the temperature coefficient of electrical resistance of iron is abnormally high. So far as I know no one has pushed his observations to the temperature at which iron ceases to be magnetic.

' The accompanying curve shows the results of experiments made upon a very soft iron wire. The abscissæ are the temperatures as estimated by the resistance of a copper wire, the ordinates represent the resistance of the iron wire having unit resistance at 20° C. It

will be seen that the temperature coefficient of iron ranges from 0·0048 at the ordinary temperature to 0·018 at a temperature just short of 855° C.; it then suddenly changes to about 0·0067. The last coefficient can only be regarded as a somewhat rough estimate.

This temperature being a higher temperature than I had observed previously in any case as the temperature at which a sample of iron ceases to be magnetic, it appeared desirable to ascertain whether the iron wire differed from other samples in this respect. A ring was formed of the wire, and was wound with a primary and secondary coil, and the resistance of the secondary was determined when the magnetisability of the iron disappeared. It was found that this resistance was the resistance which corresponded to a temperature of 870° C.; this temperature agrees with that at which the discontinuity in the resistance curve occurs, within the limits of errors of observation.

Presents, March 21, 1889.

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March 28, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Structural Arrangement of the Mineral Matters in Sedimentary and Crystalline Pearls." By GEORGE HARLEY, M.D., F.R.S. Received March 6, 1889.

[Publication deferred.]

II. "On the descending Degenerations which follow Lesions of the Gyrus marginalis and Gyrus fornicatus in Monkeys." By E. P. FRANCE. With an Introduction by Professor SCHÄFER, F.R.S (from the Physiological Laboratory, University College, London). Received March 9, 1889.

(Abstract.)

This paper contains a minute account of the descending degenerations which have been observed to make their appearance in the lower portions of the central nervous system, as the result of artificially established lesions of parts of the cerebral cortex. The work has been carried out by Mr. France with material supplied by the researches of Professor Horsley, Dr. Sanger Brown, and Professor Schäfer,

which have been published in the 'Philosophical Transactions' (vol. 179). It is illustrated partly by representations of certain of the brains showing the extent of the lesions, partly by photographs of microscopic sections through the spinal cord and medulla oblongata.

III. "On certain Ternary Alloys. I. Alloys of Lead, Tin, and Zinc." By C. R. ALDER WRIGHT, D.Sc., F.R.S., Lecturer on Chemistry and Physics, and C. THOMPSON, F.C.S., F.I.C., Demonstrator of Chemistry, in St. Mary's Hospital Medical School. Received March 5, 1889.

It is well known, that quite apart from a tendency to separate more or less completely into different mixtures during solidification, certain mixtures of molten metals show a tendency to separate into two alloys of different densities on standing fused for some time. Lead and zinc and bismuth and zinc have been shown by Matthiessen and von Bose ('Roy. Soc. Proc.,' vol. 11, p. 430) to form two such mixtures. We find that aluminium and zinc or aluminium and bismuth also behave in the same way. In each case two different alloys are formed, one consisting of the heavier metal with a little of the lighter one dissolved therein, the other of the lighter metal containing a small quantity of the heavier one.

On the other hand, *tin* will alloy indefinitely in all proportions with any one of the four metals, lead, bismuth, zinc, or aluminium, the mixtures exhibiting no particular tendency to separate into two different alloys on simply remaining at rest in a fused condition, although in certain cases more or less separation is apt to occur *during solidification*, owing to partial formation of eutectic alloy. On quickly cooling a mass of 60 to 80 grams of mixed metal, fused in a small narrow crucible and kept molten for some hours, an ingot is obtained, the highest and lowest portions of which exhibit sensibly the same composition on analysis; no more difference being observed than may reasonably be attributed to surface oxidation and volatilisation whilst standing molten, and to incipient formation of eutectic alloy during the act of solidification. Thus the following figures were obtained with two ingots of zinc and tin, and similarly in the other cases:—

	Zinc.	Tin.	Zinc.	Tin.
Top layer	31·13	68·87	61·14	38·86
Bottom layer	31·37	68·63	60·54	39·46
Mean	31·25	68·75	60·84	39·16

Various other metals, *e.g.*, cadmium, antimony, silver, &c., appear to behave like tin in this respect.

It occurred to us that it would be of interest to examine the behaviour under similar conditions of ternary mixtures where two of the ingredients are not miscible together in all proportions (like aluminium and lead), whilst the third is miscible indefinitely with either of the other two (like tin). *A priori*, it would seem probable that such mixtures would behave in a fashion similar to mixtures of alcohol, ether, and water. It is well known that these three fluids can be mixed together in certain proportions so as to form a single homogeneous liquid, not separating into two different layers on standing; whilst, on the other hand, certain mixtures of alcohol and ether, when agitated with water, ultimately form two different fluids, the heavier one consisting of water containing in solution some alcohol and ether, the lighter one of ether retaining the rest of the alcohol and some water. By parity of reasoning it might be expected that with certain proportions a single stable alloy would result, whilst with others the mass would divide into two different ternary mixtures. In point of fact this is precisely what occurs.

For a variety of reasons we selected the alloys of lead, tin, and zinc for our first experiments: these metals are easily obtained in quantity and of fair purity; the mixtures are fusible at temperatures easily attained and controlled; and the analysis of the resulting alloys is comparatively simple and easily executed with accuracy, no unimportant point when some 200 to 300 different alloys are to be examined, as we found to be ultimately necessary. Our first preliminary experiments indicated that when the lead and zinc are present in proportions not widely different (between the limits 3 to 1 and 1 to 3), the quantity of tin requisite to prevent separation into two different alloys was from $\frac{3}{8}$ to $\frac{2}{5}$ (38 to 40 per cent.) of the total mass, *i.e.*, such mixtures, after standing quiescent in a molten state for several hours and then cooled, gave ingots exhibiting sensibly the same composition at top and bottom. For example, the following figures were obtained with five different mixtures where the tin was always not less than 35 per cent. of the whole, whilst the zinc and lead varied in their ratio between the limits 1 to 2 and 2 to 1 or thereabouts:—

	Tin.	Lead.	Zinc.
Top	35·36	44·47	20·17
Bottom	35·80	43·62	20·58
Mean.......	35·58	44·05	20·37
Top	36·81	39·78	23·41
Bottom	37·91	39·41	22·68
Mean.......	37·36	39·60	23·04
Top	36·93	30·96	32·11
Bottom	38·23	31·84	29·93
Mean.......	37·58	31·40	31·02
Top	36·43	26·90	36·67
Bottom	36·90	27·93	35·17
Mean.......	36·67	27·41	35·92
Top	36·06	22·12	41·82
Bottom	37·58	23·05	39·37
Mean.......	36·82	22·59	40·59

These figures are the results of the analysis of the ingots obtained by melting the various mixtures in a crucible, well stirring together for some minutes, pouring into the red-hot bowl of a plugged clay tobacco-pipe, and keeping molten for some hours whilst at rest, and finally quickly cooling by removing the source of heat (a bunsen burner). The differences between the compositions of the top and bottom portions of the ingots are obviously no greater than what may be reasonably ascribed to surface oxidation and volatilisation, and possible slight variation introduced through partial separation into differently fusible alloys during solidification.

On the other hand, the following figures were obtained with three other mixtures, in which the tin constituted respectively about 9, 20, and 27 per cent. of the whole, whilst the zinc and lead were in the ratios 2 to 1, 1 to 1, and 1 to 2 in the three cases respectively:—

	Tin.	Lead.	Zinc.
Top	10·31	2·90	86·79
Bottom	7·70	90·22	2·08
Top	21·89	8·12	69·99
Bottom	18·48	75·76	5·76
Top	24·15	9·00	66·85
Bottom	29·56	53·20	17·24

These and various other similar experiments led us to the conclusion that the greater the proportion of tin present (provided it does not exceed the limiting amount beyond which no separation takes place) the more zinc is contained in the heavier alloy, and the more lead in the lighter one; but that the distribution of the tin throughout the entire mass is by no means uniform, the lighter alloy containing the greater percentage when the proportion of tin in the total mass is low, and *vice versâ* when it approaches towards the limiting amount; so that with a particular proportion of tin in the total mass uniform distribution as regards weight percentage occurs, but with no other proportion.

These first indications appeared to be of sufficient interest to be worth following up by the examination of a large number of mixtures so as to enable curves to be drawn representing the variations in composition of the heavier and lighter alloys relatively to one another and in the distribution of the tin throughout the compound mass. Accordingly, we first of all attempted to find out whether a moderately large variation in the temperature at which the mass was kept molten had any great influence on the end result; for if not, obviously much laborious work would be saved, thermostats and arrangements for keeping constant temperatures for long periods of time together and such like devices, involving much complexity of working, being far less indispensable than would otherwise be the case. For this purpose we prepared two series of mixtures, in each of which equal quantities of lead and zinc were weighed up with varying quantities of tin. The metals were melted in a crucible (previously heated to a dull red heat) with a little cyanide of potassium, well stirred together with a clay rod for some minutes, and then poured into the red-hot bowl of a plugged clay tobacco-pipe and kept therein molten for four to five hours. In the first series the bowl was kept hot in the flame of a bunsen burner lapping all round the bowl; in the second, the heat was intensified by surrounding the bowl with a clay cylinder so as to jacket it. A bundle of pieces of thick platinum rod heated in exactly the same way and transferred to a calorimeter, gave with bowls not

jacketted temperatures varying in different experiments between 550° and 580°, and averaging exactly 565°; whilst when heated in pipe bowls jacketted with the cylinder, temperatures were indicated varying between 675° and 705°, and averaging 689°, or 124° higher than before.* It may therefore be fairly assumed that the average temperature at which the mixtures were kept molten was not far from 124° higher in the second series than in the first.

After the requisite time had elapsed the lamp was removed, as also the jacketted cylinder when employed; in a few minutes the contents of the pipe-bowl were solid, when the clay was broken away from the somewhat conical compound ingot formed. After filing away about a couple of millimetres from the outside, the top and bottom portions were cut off by a cold chisel or saw and analysed. Usually a well-marked line of separation between the heavier and lighter alloys formed and was easily distinguishable; as long as the tin percentage in the total mass was low, this line was approximately in the middle of the mass, but with larger proportions of tin the dividing line gradually rose until the limit was being approached, beyond which no separation took place, when the dividing line was so near to the upper surface as to render it impossible to saw off even a thin layer of lighter alloy without intermixture with more or less of the heavier one.

The analysis in all cases was made as follows: a weighed portion (usually from 5 to 8 grams) was boiled with diluted pure nitric acid in a well-covered capacious beaker until complete oxidation was effected; the liquid was then diluted and allowed to stand till cold and filtered. No appreciable quantity of tin was ever found in solution.† The filtrate was evaporated to a small bulk with excess of pure sulphuric acid, and the lead sulphate produced collected and weighed; the filtrate and washings from this sometimes contained traces of lead; if so, these were precipitated by sulphuretted hydrogen

* The calorimeter employed contained a litre of water and had a water equivalent of 1050 grams; the thermometer read to $\frac{1}{100}$ of a degree centigrade. Taking the initial temperature of this calorimeter as t_1, and the final as t_2 (corrected for radiation, &c.), W as the weight of platinum, and S its mean specific heat between t_2 and T, the temperature to be measured, the value of T was calculated by the formula

$$T = \frac{1050 \times (t_2 - t_1)}{W . S} + t_2.$$

W in most of the observations was 51·055 grams; S was taken from Pouillet's values by interpolation as being

Between t_2 (about 15°) and		565	= 0·03545
„	„	689	= 0·03595
„	„	750	= 0·03625

† When tin is oxidised by dilute nitric acid in presence of relatively large quantities of certain metals, especially iron (*e.g.*, in the case of tinplate), very perceptible quantities of tin are generally taken permanently into solution.

and determined. Owing to the solution of lime, alumina, &c., from the vessels used during evaporation, we found that too high a value was always obtained by directly precipitating zinc with sodium carbonate from the acid fluid; wherefore we first precipitated it as sulphide by means of ammonia and ammonium sulphide, and redissolved the precipitate in dilute hydrochloric acid (after collection on a filter) before precipitating as carbonate. Any traces of zinc not thrown down thus were precipitated by adding a few drops of ammonium sulphide to the filtrate and thus estimated, whilst any traces of alumina and iron contained in the zinc oxide were determined and subtracted (after weighing) by dissolving in hydrochloric acid and supersaturating with ammonia. Usually these various filtrate and other corrections were all but infinitesimal: the analyses generally added up to 99·8 to 99·9 per cent., and the percentages finally quoted are usually reckoned upon the sum of the tin, lead, and zinc found as 100; in some cases, more especially with the lighter alloys, the tin and lead only were determimed, the zinc being taken by difference.

Forty compound ingots (20 in each series) thus treated gave numbers concording together reasonably well, furnishing the following averages; in several cases duplicate ingots were prepared, the mean values being those quoted.

Series I.—Temperature near 565°.

Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.	
0	98·70	1·30	0	1·10	98·90	0
1·79	96·86	1·35	2·49	1·80	95·71	0·70
4·67	93·69	1·64	7·31	3·17	89·52	2·64
7·66	89·77	2·57	10·97	4·67	84 36	3·31
8·99	88·04	2·97	13·04	5·70	81·26	4·05
12·15	84·19	3·66	16·50	6·28	77·22	4·35
16·66	78·98	4·36	20·63	7·10	72·27	3·97
18·10	76·57	5·33	21·75	7·30	70·95	3·65
21·36	71·34	7·30	24·09	8·58	67·33	2·73
25·81	61·33	12·86	26·28	9·92	63·80	0·47
29·60	54·90	16·50	27·91	10·95	61·14	−1·69
33·69	45·05	21·26	29·68	13·46	56·86	−4·01
34·98	49·37	24·65	29·84	14·26	55·90	−5·09
36·85	35·25	27·90	34·57	30·12	35·31	—

In each series the last mixture yielded so thin a layer of lighter alloy that it was impossible to saw off a sample free from admixture with a large amount of heavier alloy.

Series II.—Temperature near

Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.	
4·67	93·44	1·89	6·91	2·90	90·19	2·24
8·79	89·61	1·60	12·60	4·50	82·90	3·81
14·38	80·65	4·97	18·33	6·14	75·53	3·84
15·66	79·24	5·10	19·96	6·36	73·68	4·30
19·25	74·80	5·95	22·70	8·39	68·91	3·45
23·30	66·39	10·31	26·04	9·59	64·37	2·74
25·00	62·30	12·70	26·42	9·81	63·77	1·42
26·68	58·16	15·16	27·64	10·50	61·86	0·96
28·59	55·16	16·25	28·23	12·10	59·67	−0·36
31·11	49·92	18·97	27·76	11·68	60·56	−3·35
33·24	44·80	21·96	28·73	12·53	58·74	−4·51
34·99	37·67	27·34	29·20	14·45	56·35	−5·79
36·48	33·68	29·84	34·50	29·79	35·71	—

Besides these forty ingots, however, a number of others were obtained, yielding numbers on analysis not agreeing at all well with the forty, more especially as regards the lighter alloys. In all cases the figures were just such as would be obtained if complete separation by gravitation had not taken place, so that the lighter alloy retained a little of the heavier one disseminated through it, and *vice versâ;* just as when ether and certain aqueous liquids are agitated together, a kind of froth forms, which takes a long time before it entirely separates, forming two clear liquids. As the experiments subsequently detailed render it certain that these abnormal ingots were simply cases where the separation was imperfect, the figures obtained with them are not included in the above tables, but it is obvious that this same source of error may have applied to a lesser extent even to some of the forty ingots not rejected. No doubt this was actually the case in some instances, on which account the curves obtained on plotting the above figures show a certain amount of sinuosity and irregularity. The error from this cause, however, as subsequently shown, is not serious; so that the concordance between the curves got from the two series of analyses respectively is sufficiently marked to show that no very considerable influence is exerted by a difference of temperature amounting to some 124°, on the way in which a given mass of lead, tin, and zinc divides itself on standing molten.

Three noteworthy curves are thus obtainable:—

(*a*.) When the tin percentages in the heavier alloy are plotted as abscissæ and the zinc percentages as ordinates.

(*b.*) When the tin percentages in the lighter alloy are plotted as abscissæ and the lead percentages as ordinates.

(*c.*) When the tin percentages in the heavier alloy are plotted as abscissæ and excesses of the percentage (+ or −) in the lighter alloy over those in the heavier one as ordinates.

These three curves respectively represent approximately the solubility of zinc in lead containing tin and that of lead in zinc containing tin, and the relative distribution of tin in the two alloys formed

simultaneously. Fig. 1 represents the two curves of the first kind plotted respectively from Series I and Series II, the continuous line

representing the first and the dotted line the second. Fig. 2 represents the corresponding curves of the second kind, and fig. 3 those of the third kind. Obviously, in each case there is little difference between the continuous and dotted lines, so that it may be fairly concluded that the effect of variation in temperature from 565° to 689° is negligible as compared with the experimental errors, more especially those due to imperfect separation by gravitation of the two alloys from one another.

The curves representing the distribution (fig. 3) are remarkable. As long as the tin percentage in the total mass is less than about sixteen the lighter alloy contains more tin than the heavier one; at about this point (representing some 14 per cent. in the heavier and 18 per cent. in the lighter alloy) the difference becomes a maximum after which the difference diminishes, until at about 28 per cent. the same percentage of tin is contained in both alloys. After this the heavier alloy contains more tin than the lighter, the difference continually increasing.

Causes retarding Separation of Lighter and Heavier Alloy.

Before proceeding further we thought it desirable to trace out the causes rendering separation incomplete, even after some hours' standing. At first we attributed it to the influence of traces of impurities in the metals used, but on repeating the observations in pipe-bowls with the purest samples of each metal obtainable, we still occasionally got irregular results, showing that this was not the sole cause. Next we thought that the error might be due to the partial formation of eutectic alloys during solidification, in such a way that

the portions of the ingots analysed did not truly represent the composition of the lighter and heavier alloys whilst molten. To avoid this we devised an arrangement whereby we could draw samples from the molten mass whilst still fluid. This consisted of a crucible holding some 500 to 600 grams of molten alloy, with a hole drilled through the bottom, closed by a conical plug worked up and down by means of a screw and lever. At the required moment, by turning the lever the plug could be slightly raised by the screw, so that a little of the lowest layer of molten metal escaped through the valve thus opened into an ingot mould placed to receive it. Simultaneously the top layer of molten metal could be sampled by a hot porcelain spoon. The crucible was surrounded by a cylindrical clay jacket to keep in the heat, which was supplied by a number of bunsen burners arranged so as to form a ring of flame between the crucible and jacket. Apparently there was no reason whatever why this arrangement should not work successfully; but in practice we did not succeed in getting, by its means, any results at all on which reliance could be placed. The analyses indicated that instead of this arrangement giving more complete separation than the pipe-bowls, it hardly ever gave so complete a one; on drawing samples at different times, instead of the separation gradually becoming more perfect as time elapsed, the opposite was often the case, some intermixing influence being apparently at work, which frequently was more powerful than the effect of gravitation in causing the lighter and heavier alloys to separate from one another. For instance, the following numbers were obtained in one experiment, indicating very incomplete separation as compared with the pipe-bowl results:—

Time.	Heavier alloy.			Lighter alloy.		
	Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.
5 hours	15·01	76·60	8·39	18·19	12·69	69·12
10 "	14·39	74·86	10·75	18·27	20·86	60·87

In another case no sensible separation at all was brought about after either four or eight hours with a mixture that separated readily when fused in a tall narrow crucible.

These particular two experiments are extreme cases as to irregularity; but still, in almost every instance, the figures obtained with the valve crucible were such as to show that the separation of heavier and lighter alloys from one another therein was extremely imperfect.

Valve Crucible.

	After 4 hours.			After 8 hours.		
	Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.
Top	25·30	24·48	50·22	26·30	24·16	49·30
Bottom	25·71	24·71	49·58	25·82	25·02	49·16

Tall narrow Crucible: 8 hours.

	Tin.	Lead.	Zinc.
Top	27·83	9·81	62·36
Bottom	25·63	62·39	11·98

In these cases it was clear that the formation of eutectic alloys during solidification was not the cause of the irregularities observed, whence presumably the same is true for the less irregular results obtained with the pipe-bowls. Ultimately we traced the cause to convection currents set up through unequal heating of the walls of the containing vessels at different levels, and found that the imperfect separation could be almost completely obviated by so heating the mass as to avoid this inequality of temperature. This we ultimately effected by employing crucibles very long in proportion to their diameter (large test-tubes moulded on a core from a plastic mixture of fireclay and syrupy silicate of soda, diluted with about three times its weight of water), heated by immersion in a bath of molten lead some 8 or 9 inches deep, contained in an iron cylindrical vessel (the lower two-thirds of a mercury bottle), surrounded by a concentric clay jacket and heated by a number of bunsen burners playing into the annular interspace. The molten metals being well intermixed in a crucible (with a little potassium cyanide), the mixture was poured into a red-hot clay test-tube, which was then quickly transferred to the hot lead-bath, the mouth of the tube being covered with a heavy iron cap so as to depress the test-tube into the lead to such a depth that the top of the molten metal inside was some 2 inches below the surface of the lead in the bath whilst the tube was kept vertical. Under these conditions, practically complete separation was always brought about after six to eight hours in the lead-bath. Usually several test-tubes were heated simultaneously. After the required time had elapsed they were carefully lifted out without shaking, and set by to cool, still in a vertical

position. To diminish oxidation, a reducing atmosphere was maintained in the upper part of the lead-bath by covering it loosely with a lid and passing a jet of coal-gas inside. In some few instances the test-tubes were not weighted down with iron caps, so that the level of the metal inside was *above* the top of the lead; under those circumstances the upper part of the metal was largely heated by convection, and in all such cases it was found that the lighter alloy retained some of the heavier alloy interspersed through it, the convection currents preventing complete separation by gravitation. Thus, for example, two similar mixtures, containing about 23·5 per cent. of tin, were heated simultaneously for eight hours, one completely depressed so as to avoid convection currents, the other raised so as to ensure their production, with the following results:—

	Heavier alloy.			Lighter alloy.		
	Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.
Depressed....	22·32	69·84	7·84	25·71	8·84	65·44
Raised.......	22·50	68·89	8·61	24·16	17·44	58·40

The figures obtained with the depressed tube are perfectly normal (*vide infra*), whilst those obtained with the other are such as to show that, owing to the convection currents set up, a little of the lighter alloy was still intermixed with the heavier, whilst a considerable amount of the heavier one was interspersed throughout the lighter one.

Lead-bath Observations.

As a check on the curves above described obtained with pipe-bowls, we made another similar series of observations with mixtures containing equal quantities of lead and zinc and varying proportions of tin, employing clay test-tubes heated in a lead-bath. The temperature of the bath was ascertained from time to time by heating a bundle of thick platinum rods in a clay test-tube in the bath and transferring it to the calorimeter; the temperatures thus observed lay between 610° and 710°, with an average of 646° for the whole series of ingots, twenty-two of which were prepared, not one giving any markedly abnormal results on analysis. The following figures were obtained, several of the ingots being duplicates and the mean figures being quoted. In all cases the time during which the mass remained molten was about eight hours.

Series III.—Lead-bath. Temperature near 646°.

Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.	
0	98·70	1·30	0	1·17	98·83	0
4·77	93·67	1·56	7·17	2·17	90·66	2·40
6·42	91·96	1·62	9·81	2·78	87·41	3·39
9·85	87·70	2·45	13·36	3·49	83·15	3·51
13·06	83·70	3·24	17·13	4·04	78·83	4·07
13·77	82·42	3·81	18·37	4·54	77·09	4·60
15·30	79·90	4·80	19·61	5·29	75·10	4·31
22·32	69·84	7·84	25·72	8·84	65·44	3·40
26·99	60·08	12·93	28·22	10·49	61·29	1·23
28·57	56·51	14·92	28·55	11·63	59·82	−0·02
30·87	52·47	16·66	28·90	11·66	59·44	−1·98
32·86	46·94	20·20	30·13	13·19	56·68	−2·73
34·76	42·43	22·81	30·19	13·80	56·01	−4·57
35·38	40·07	24·55	29·76	13·81	56·43	−5·62

On plotting these numbers as before, it is at once evident that the curves thence obtained differ but little from those obtained in Series I and II, excepting in being more regular, what differences exist being such as are obviously due to the more nearly complete separation now obtained in all cases; whence it may reasonably be inferred that the conclusion previously arrived at is correct, viz., that a variation in temperature between 565° and 689° makes practically no difference in the way in which a given mass of metal divides itself on standing molten. This conclusion is corroborated by the results described below, obtained with two other series of mixtures containing lead and zinc in the proportion 1 to 2, in the first of which a mean temperature close to 650° was employed, and in the second a temperature about 100° higher; the curves deduced from the two series respectively differing from one another only by amounts barely, if at all, outside the limits of experimental error.

Experiments with Lead and Zinc in Unequal Proportions.

We next made several series of observations with lead and zinc in unequal proportions and varying quantities of tin, with the object of finding out how far the distribution of tin between the two resulting alloys is influenced by the relative masses of metals present. If the alloys formed when completely separated from one another are respectively saturated solution of zinc in lead containing tin (bottom) and saturated solution of lead in zinc containing tin (top), it should result that the two first curves obtained as above described will be the

same no matter what may be the relative proportions of zinc and lead in the total mass; and this, in point of fact, we find to be the case. But it does not follow therefrom that with a mass of metal containing x per cent. of tin the same pair of alloys will be obtained, no matter in what relative proportions the zinc and lead may exist in the remaining $100-x$ per cent.; and, in point of fact, we find not only that a different pair results for each variation in the relative proportions of zinc and lead in such a case, but, further, that the curves representing the relative distribution of tin in the two alloys are not the same for all proportions between zinc and lead in the total mass. When zinc predominates the curve rises less rapidly, the maximum difference in tin percentage is attained later, and the point of equal distribution of tin throughout the entire mass lies further from the origin than when the zinc and lead are present in equal proportions in the entire mass; and *vice versâ* when lead predominates.

Thus the following values were obtained from a series of sixteen compound ingots, prepared in the lead-bath at an average temperature of close to 650°, the masses remaining fused for about eight hours in each case, the proportions between zinc and lead in the metals weighed up being uniformly 2 to 1.

Series IV.—Zinc double the Lead present. Temperature near 650°.

Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.	
0	98·78	1·22	0	1·08	98·92	0
3·67	94·39	1·94	5·17	2·21	92·62	1·50
7·70	90·22	2·08	10·31	2·90	86·79	2·59
10·81	86·42	2·77	14·74	3·71	81·55	3·93
14·85	81·40	3·75	19·48	5·22	75·30	4·63
16·36	79·24	4·40	21·09	5·97	72·94	4·73
18·79	75·62	5·59	23·07	6·67	70·26	4·28
20·19	74·03	5·78	24·11	6·98	68·91	3·92
25·63	62·39	11·98	27·83	9·81	62·36	2·20
28·70	54·76	16·54	28·80	10·76	60·44	0·10
30·58	51·01	18·41	29·39	11·82	58·79	−1·19
..	..	..	29·80	12·24	57·96	..
33·49	44·21	22·30	31·35	13·55	55·10	−2·14
35·34	37·06	27·60	32·03	15·57	52·40	−3·31

A similar series of eight ingots at a higher temperature, close to 750°, gave the following results:—

Series V.—Zinc double the Lead present. Temperature near 750°.

Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.	
5·96	92·10	1·94	8·53	3·18	88·29	2·57
10·76	86·29	2·95	14·87	3·69	81·44	4·11
16·58	78·43	4·99	20·44	6·00	73·56	3·86
22·57	70·79	6·64	25·41	7·05	67·54	2·84
29·41	52·75	17·84	29·34	11·71	58·95	−0·07
31·95	46·73	21·32	31·02	13·49	55·40	−0·93
34·24	42·28	23·48	31·70	13·74	54·56	−2·54
34·78	40·23	24·99	32·16	15·21	52·63	−2·62

On plotting these two series it is obvious that the curves are practically identical in both cases; indicating, as above shown, that little, if any, sensible difference is brought about by a considerable temperature variation in the way in which a given mass of metal divides itself on standing. On the other hand, whilst the solubility curves of zinc in lead-tin and of lead in zinc-tin thence derived are sensibly the same as those derived from Series III, the tin distribution curves are by no means identical therewith, especially with the highest tin percentages.

Another series of mixtures was then prepared with zinc and lead in the proportions 1 to 2. The following values were obtained from twenty ingots fused about eight hours at a temperature close to 650°.

Series VI.—Lead double the Zinc present. Temperature near 650°.

Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.	
0	98·86	1·14	0	1·22	98·78	0
5·38	92·82	1·80	8·20	2·25	89·55	2·82
9·06	89·03	1·91	12·44	3·36	84·20	3·38
11·27	86·23	2·50	14·94	3·92	81·14	3·67
15·40	80·78	3·82	18·70	5·19	86·11	3·30
19·92	74·11	5·97	..	..	..	..
21·24	71·70	7·06	23·32	7·38	69·30	2·08
22·78	68·85	8·37	24·21	7·80	67·99	1·43
25·28	64·00	10·72	25·31	8·29	66·40	−0·03
26·48	58·96	14·56	24·67	7·95	67·38	−1·81
29·66	54·99	15·35	25·03	8·20	66·77	−4·63
31·02	50·92	18·06	25·40	8·70	65·90	−5·62

. On plotting these numbers it becomes evident that whilst the solubility curves of zinc in lead-tin and of lead in zinc-tin thence derived are sensibly identical with those obtained from Series III, IV, and V, the tin distribution curve is different from either of those obtained with zinc and lead in the proportions 1 to 1 and 2 to 1.

Fig. 4 represents the mean solubility curves (1) for zinc in lead-tin, (2) for lead in zinc-tin, derived from all the foregoing observations

made in the lead-bath, the abscissæ being in each case tin percentages, and the ordinates zinc percentages for curve No. 1, and lead percentages for curve No. 2. The two curves are not widely different at first, but latterly curve No. 1 distinctly overlies No. 2.

Fig. 5 represents the mean tin distribution curves derived from the preceding observations, No. 1 being that derived from the experiments where the zinc present was double the lead, No. 2 where the two metals were in equal proportions, and No. 3 where the lead was double the zinc. The maxima obtained correspond respectively with the ordinate values 4·7, 4·6, and 3·7, and are situated at about the abscissa values 15, 14, and 12 respectively. The crossing points (points of equal tin distribution as regards weight percentage through the mass) are respectively close to 29·0, 28·5, and 25·2 per cent. of tin. The gradient of rise towards the maximum and of fall subsequently below the crossing point is steepest in curve No. 3 and least steep in No. 1.

The following tables contain the mean values graphically represented by these curves.

Solubility of Zinc in Lead-tin.			Solubility of Lead in Zinc-tin.		
Per cent. of tin.	Per cent. of zinc.	Difference.	Per cent. of tin.	Per cent. of lead.	Difference.
0	1·24	0·20	0	1·14	0·33
2	1·44	0·21	2	1·47	0·33
4	1·65	0·24	4	1·80	0·33
6	1·89	0·26	6	2·13	0·33
8	2·15	0·30	8	2·46	0·34
10	2·45	0·40	10	2·80	0·34
12	2·85	0·55	12	3·14	0·36
14	3·4	0·7	14	3·50	0·40
16	4·1	0·9	16	3·9	0·6
18	5·0	1·1	18	4·5	0·8
20	6·1	1·4	20	5·3	1·0
22	7·5	1·75	22	6·3	1·2
24	9·25	2·0	24	7·5	1·4
26	11·25	2·25	26	8·9	1·7
28	14·5	2·5	28	10·6	2·15
30	17·0	2·75	30	12·75	2·75
32	19·75	3·25	32	15·5	
34	23·0	4·0			
36	27·0				

Tin Distribution Curves.

Percentage of tin in heavier alloy.	Excess of tin percentage in lighter alloy over that in heavier.					
	Zinc double the lead.		Equal.		Lead double the zinc.	
		Difference.		Difference.		Difference.
2	0·8	0·8	0·9	0·9	1·1	1·1
4	1·6	0·8	1·8	0·9	2·2	1·1
6	2·4	0·8	2·7	0·9	2·9	0·7
8	3·1	0·7	3·4	0·7	3·3	0·4
10	3·7	0·6	3·9	0·5	3·55	0·25
12	4·2	0·5	4·35	0·45	3·7	0·15
14	4·55	0·35	4·6	0·25	3·55	−0·15
15	4·7	0·15	..	..	..	..
16	4·55	−0·15	4·35	−0·25	3·3	−0·25
18	4·2	−0·35	4·0	−0·35	3·0.	−0·3
20	3·75	−0·45	3·6	−0·4	2·5	−0·5
22	3·1	−0·65	3·0	−0·6	1·75	−0·75
24	2·3	−0·8	2·2	−0·8	0·75	−1·0
25·2	..	..	..	..	0	..
26	1·4	−0·9	1·3	−0·9	−0·8	−1·55
28	0·45	−0·95	0·25	−1·05	−2·7	−1·9
28·5	..	..	0	..	..	..
29	0	..	..	..	..	..
30	−0·5	−0·95	−1·10	−1·35	−4·9	−2·2
32	−1·5	−1·0	−2·66	−1·56	..	..
34	−2·5	−1·0	−4·33	−1·67	..	..
36	−3·5	−1·0	−6·0	−1·67	..	..

Miscibility of Lead and Zinc in the Absence of Tin.

It is worth noticing that four sets of ingots have been examined above, with the following results:—

	Heavier alloy.		Lighter alloy.	
	Lead.	Zinc.	Lead.	Zinc.
Pipe-bowl	98·70	1·30	1·10	98·90
Lead-bath, zinc double the lead	98·78	1·22	1·08	98·92
" equal	98·70	1·30	1·17	98·83
" lead double the zinc	98·86	1·14	1·22	98·78
Mean	98·76	1·24	1·14	99·86

These values are somewhat lower as regards the zinc dissolved by lead, and *vice versâ*, than the figures given by Matthiessen and

v. Bose (*loc. cit. supra*), which lead to the percentages 1·62—1·79 of zinc in heavier alloy, mean = 1·67; and 1·17—1·22 of lead in lighter alloy, mean = 1·20. Obviously this arises from the fact that the method of working adopted by Matthiessen and v. Bose did not allow of so complete a separation taking place as was effected in our experiments, as they employed a far shorter time.

Variation in mean Composition through Oxidation and Volatilisation.

In melting and mixing together the metals employed, it is quite impossible to avoid some loss by oxidation, even when a luminous gas flame is directed into the crucible so as to maintain a reducing atmosphere therein. Moreover, some amount of volatilisation, especially of zinc, takes place, owing to prolonged heating at 650—750°. A number of observations made with mixtures that did not separate into two alloys showed that the total quantity of tin in the final ingot is but little, if at all, less than that originally weighed up; some lead is lost and more zinc, roughly averaging about twice as much as the lead. The total amount of loss, however, even after eight hours' heating, is not very great; as a rule, ingots were made for which about 80 grams of total metal were weighed up; the weight of final compound ingot was generally near to 77 grams (excluding mechanical losses during stirring and transference to the clay test-tubes), about 3 grams representing the loss by oxidation and volatilisation. In a few cases a larger amount of oxidation took place, but comparatively rarely with careful handling. The result of this action is to cause an increment in the mean percentage of tin in the mass to the extent of something like one twenty-fifth of its value (*i.e.*, a mass originally containing 25 per cent. of tin will ultimately contain about 26). Simultaneously the ratio of zinc to lead is altered; only to a small extent if these metals were originally in the proportion of 2 to 1, but relatively more if the zinc were present to a lesser extent.

In the foregoing experiments the proportions subsisting between zinc, lead, and tin referred to are uniformly those in which the metals were *weighed up for use*, and consequently not quite the same as those actually subsisting in the compound ingots finally obtained; these latter probably contained zinc and lead in ratios near to 2 to 1, 0·96 to 1, and 0·46 to 1, respectively, on the average.

Summary of Results.

When a mixture of lead, tin, and zinc in the molten condition is well stirred up by mechanical means and then left to itself for some hours at as nearly as possible a uniform temperature, a single homogeneous alloy results if the proportion of tin present is not less than three-eighths of the whole; but if materially less tin than this is

present, the mass divides itself into two different ternary alloys, lead predominating in the heavier one and zinc in the lighter one. This phenomenon is entirely distinct from the segregation of alloys during solidification, in consequence of formation of eutectic or other differently fusible alloys.

If there is little or no inequality of temperature at different parts of the mass, separation by gravitation only is complete in a few hours, at any rate when tolerably pure metals are employed; but if the mode of heating is such that convection currents are set up, the separation is greatly interfered with, and in extreme cases almost entirely prevented.

The heavier alloy is a saturated solution of zinc in lead containing tin, and the lighter one a similar solution of lead in zinc containing tin. No matter what the relative proportions between lead and zinc in the original mass, the two alloys always correspond to two conjugate points on the solubility curves of zinc in lead-tin and of lead in zinc-tin.

But little, if any, difference in the way in which a given mass divides itself is noticeable, whether the temperature at which the molten mass is maintained is below 600° C. or above 700° C.

The tin contained in the mass does not distribute itself equally in the two alloys except when present in one particular proportion, which varies with the ratio of the zinc to the lead in the entire mass. With less tin than this the lighter alloy, and with more the heavier one, takes up the higher percentage of tin.

Curves drawn representing the tin present in the heavier alloy as abscissæ, and the (+ or −) excess of tin in the lighter alloy over that in the heavier one as ordinates, are found to differ with the ratio of zinc to lead in the entire mass. They always possess the same general features, viz., rising from the origin to a maximum elevation, then sinking down again to the base line, and crossing it so as to become negative; but the position and height of the maximum, the crossing point, and the general dimensions of the curve vary with the ratio of zinc to lead in the mass.

As a result of this, whilst an indefinite number of different mixtures may be prepared, each one of which will give the same heavier alloy, the lighter alloy simultaneously formed will be different in each case; and conversely.

When no tin is present, lead dissolves zinc to such an extent as to form an alloy containing 1·24 per cent. of zinc, and zinc dissolves lead forming an alloy containing 1·14 per cent. of lead; the higher values found by previous observers being slightly incorrect through imperfect separation.

Before attempting to theorise on the causes leading to the remarkable way in which tin is distributed in these ternary alloys, we desire

to accumulate additional data derived from the examination of other parallel cases, such as the ternary alloys obtained by adding tin to the immiscible pairs of metals, zinc and bismuth, aluminium and lead, and aluminium and bismuth; or by similarly employing other metals instead of tin. Nothing abnormal appears to characterise the solubility curves of zinc in lead-tin and of lead in zinc-tin; in each case the amount of one metal dissolved by the other increases as the quantity of tin present increases, in such a way that the curves are somewhat concave upwards.

IV. "The Diurnal Variation of Terrestrial Magnetism." By ARTHUR SCHUSTER, F.R.S., Professor of Physics, with an Appendix by H. LAMB, F.R.S., Professor of Mathematics, Owens College, Manchester. Received March 20, 1889.

(Abstract.)

In the year 1839 Gauss published his celebrated Memoir on Terrestrial Magnetism, in which the potential on the earth's surface was calculated to twenty-four terms of a series of surface harmonics. It was proved in this memoir that if the horizontal components of magnetic force were known all over the earth the surface potential could be derived without the help of the vertical forces, and it is well known now how these latter can be used to separate the terms of the potential which depend on internal from those which depend on external sources. Nevertheless, Gauss made use of the vertical forces in his calculations of the surface potential, in order to ensure a greater degree of accuracy. He assumed for this purpose that magnetic matter was distributed through the interior of the earth, and mentions the fair agreement between calculated and observed facts as a justification of his assumption. In the latter part of the memoir it was suggested that the same method should be employed in the investigation of the regular and secular variations.

The use of harmonic analysis to separate internal from external causes has never been put to a practical test, but it seems to me to be specially well adapted to inquiries on the causes of the periodic oscillations of the magnetic needle.

If the magnetic effects can be fairly represented by a single term in the series of harmonics as far as the horizontal forces are concerned, there should be no doubt as to the location of the disturbing cause, for the vertical force should be in the opposite direction if the origin is outside from what it should be if the origin is inside the earth. As the expression for the potential contains in one case the distance from the earth's centre in the numerator, in the other case in the denominator, and as the vertical force depends on the diffe-

rential coefficient with regard to the distance from the earth to the centre, each single term in the series is of opposite sign according to the location of the cause; but what is true for each single term need not be true for the sum of the series. By a curious combination of terms the vertical forces might possibly be of the same sign on whichever of the two hypotheses it is calculated. In any case, however, the differences between the two results will be of the same order of magnitude as the vertical force itself. If it were then a question simply of deciding whether the cause is outside or inside, without taking into account a possible combination of both causes, the result should not be doubtful, even if we have only an approximate knowledge of the vertical forces.

Two years ago I showed that the leading features of the horizontal components for diurnal variation could be approximately represented by the surface harmonic of the second degree and first type, and that the vertical variation agreed in direction and phase with the calculation on the assumption that the seat of the force is outside the earth. The agreement seemed to me to be sufficiently good to justify the conclusion that the greater part of the variation is due to causes outside the earth's surface. Nevertheless, it seemed advisable to enter more fully into the matter, as in the first approximate treatment of the subject a number of important questions had to be left untouched. I now publish the results of an investigation which has been carried out, as far as the observations at my disposal have allowed me to do. My original conclusions have been fully confirmed, and some further information has been obtained, which I believe to be of importance.

I have made use of the observations taken at Bombay, Lisbon, Greenwich, and St. Petersburg. The horizontal components of the diurnal variation during the year 1870 were in the first place reduced to the same system of coordinates and to the same units. If we remember that experience has shown the diurnal variation to be very nearly the same for places in the same latitude, except near the magnetic pole, and also that it is symmetrical north and south of the equator, we may for a given time of day assume the horizontal components known over eight circles of latitude, four of which are north and four south of the equator. If we chose the period of the year for which the reduction is made to be that corresponding to the summer months in the northern hemisphere, we must take the variation in the southern hemisphere to be the same as that found during the winter months north of the equator. This was done in one part of the inquiry; in the other the mean of the whole year was taken, and in that case the same values hold north and south of the equator, with the same sign for the force towards geographical north, and opposite sign for the force towards the geographical west.

From the horizontal components the potential was calculated in terms of a series of surface harmonics. It was found that in order to represent both the summer and the winter effect with sufficient accuracy thirty-eight terms were necessary. In this calculation the vertical forces were not made use of at all.

Unfortunately we do not possess complete records for the vertical force variation during the year 1870, except at Lisbon; but the type of that force is very nearly the same from year to year, varying only slightly in amplitude. It is shown that, as far as the conclusions drawn in the paper are concerned, an accurate knowledge of the amplitude of the vertical force is not required. I have chosen for comparison the vertical force of Bombay in the year 1873, and for Greenwich in 1882. As regards St. Petersburg, vertical force records exist for 1870, but they have not been corrected for temperature variations of the magnet, and are therefore of doubtful importance. I have therefore used the St. Petersburg observations for 1878, in addition to those for 1870.

From the potential, as calculated from the horizontal components, we can deduce the vertical force, either on the assumption that the variation is due to an outside cause, or that it is due to an inside cause; and compare the vertical forces thus found with the vertical forces as actually observed.

If we put both into the form

$$r_n \cos n\,(t-t_n),$$

we can obtain an idea of the agreement as regards amplitude and phase for each harmonic term. The following tables give the results for $n = 1$ and $n = 2$, that is, for the diurnal and the semi-diurnal variation.

Table I.

Observed and calculated Values of the Coefficients t_1 and t_2 of Vertical Force, when expressed in the form $r_1 \cos(t-t_1)+r_2 \cos 2(t-t_2)$, on the supposition that the Disturbing Force is *inside* the Earth.

	t_1.			t_2.		
	Calc.	Obs.	Diff.	Calc.	Obs.	Diff.
	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.
Bombay	23 02	11 13	+11 49	9 55	4 23	+5 32
Lisbon.............	22 35	10 40	+11 58	11 42	5 50	+5 52
Greenwich	22 06	8 42	−11 37	11 32	5 56	+5 36
St. Petersburg, 1870.	21 16	3 10	− 5 54	10 48	7 05	+3 43
" 1878.	..	7 05	− 9 49	..	6 12	+4 36

Table II.

Observed and calculated Values of the Coefficients t and t_2 when expressed in the form, $r_1 \cos(t-t_1)+r_2 \cos 2(t-t_2)$, on the supposition that the disturbing force is *outside* the Earth.

	t_1.			t_2.		
	Calc.	Obs.	Diff.	Calc.	Obs.	Diff.
	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.
Bombay	11 10	11 13	−0 03	3 47	4 23	−0 36
Lisbon	10 37	10 40	−0 03	5 46	5 50	−0 04
Greenwich	10 03	8 42	+1 21	5 38	5 56	−0 18
St. Petersburg, 1870	8 52	3 10	+5 42	4 38	7 05	−2 27
,, 1878	..	7 05	−1 47	..	6 12	−1 34

Table III.

Observed and calculated Values of r_1 and r_2 in the Expression $r_1 \cos(t-t_1)+r_2 \cos_2(t-t_2)$ for Vertical Force.

	r_1.			r_2.		
	Calculated from inside.	Calculated from outside.	Observed.	Calculated from inside	Calculated from outside.	Observed.
Bombay	236	144	43	171	132	35
Lisbon	491	346	176	333	277	153
Greenwich	398	269	65	143	112	51
St. Petersburg, 1870	235	142	169	77	53	71
,, 1878	..	..	30	..	..	24

In Table I the comparison of the observed phases is made with the values calculated on the assumption that the disturbing force is inside the earth. In Table II the same comparison is made on the alternative hypothesis. There is complete disagreement in Table I between the observed and calculated values, and nearly complete agreement in Table II. It is seen how both at Lisbon and Bombay the time of maximum displacement agrees within three minutes of time for the diurnal variation, and at Lisbon within four minutes of time also for the semi-diurnal variation. Considering that Lisbon is the most important station, not only on account of its geographical position, but also because the observed vertical forces apply to the

same year as the calculated ones, the result is strikingly in favour of the outside force. The results for Greenwich argue in the same direction. As regards St. Petersburg, the results for 1870 neither agree with one nor with the other hypothesis, and it has already been mentioned that the observations for 1870 are doubtful, but the results for 1878 agree well with the hypothesis of an outside disturbing force.

Table III gives the comparison for amplitude. It is seen that the observed amplitudes are throughout smaller than the calculated ones. If curves are drawn representing the results of Tables I, II, III, it is clearly seen how well the calculated vertical forces agree with the observed ones as regards phase, if we assume the cause of the variation to be outside.

If we then take it as proved that the primary cause of this variation comes to us from outside the earth's surface, we are led to consider that a varying magnetic potential must cause induced currents within the earth, if that body is a sufficiently good conductor. These induced currents might be the cause of the apparent reduction in amplitude. As my colleague, Professor Lamb, has given considerable attention to the problem of currents in a conducting sphere, I consulted him, and he gave me the formulæ by means of which the induced currents can be calculated. His investigation is given in an appendix to the paper. The result is very interesting. If the earth is treated as a conducting sphere, the observed reduction in amplitude is accounted for, but that reduction should be accompanied by a change of phase which is not given by observation. We can reconcile all facts if we assume, as suggested by Professor Lamb, the average conductivity of the outer layers of the earth to be very small, so that the reduction in amplitude is chiefly due to currents induced in the inner layers. If the conductivity inside is sufficiently large, a considerable reduction in amplitude would not be accompanied by a sensible change of phase. We have arrived, therefore, at the following result:—

The vertical forces of the diurnal variation can be accounted for if we assume an outside cause of the variation, which induces currents in the earth, and if the earth's conductivity is greater in the lower strata than near the surface.

Professor Balfour Stewart's suggestion that convection currents in the atmosphere moving across the lines of the earth's magnetic forces are the causes of the daily variation, gains much in probability by this investigation. If the daily variation of the barometer is accompanied by a horizontal current in the atmosphere similar to the tangential motion in waves propagated in shallow canals, and if the conductivity of the air is sufficiently good, the effects on our magnetic needles would be very similar to those actually observed. The difficulty as to the conductivity of the air is partly met by the author's

investigation of the behaviour of gases through which electric discharges are passing.

It will be interesting to follow out the investigation, especially with a view of examining the influence of sun-spot variation. The question of magnetic disturbances is more complicated, but as magnetical observatories are being established in many countries, the time may not be far distant when we shall be able to bring the irregular disturbances within the reach of calculation.

In order to facilitate the necessarily long computations, the author makes an appeal to the heads of magnetic observatories to reduce the regular variation according to the method adopted by Wild at St. Petersburg, or that in use at Greenwich, the two being nearly identical. The variations should also be reduced to the geographical coordinates, instead of to magnetic coordinates.

The author acknowledges the help he has received from Mr. William Ellis in some of the reductions; he has also to thank his assistant, Mr. A. Stanton, for much labour bestowed on making and checking numerical calculations.

V. "On the Conditions for effective Scour in Drain-pipes of Circular Section." By HENRY HENNESSY, F.R.S., Professor of Applied Mathematics and Mechanism in the Royal College of Science for Ireland. Received March 1, 1889.

Presents, March 28, 1889.

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"The Spinal Curvature in an Aboriginal Australian." By D. J. CUNNINGHAM, M.D. (Edin. and Dubl.), Professor of Anatomy in the University of Dublin. Communicated by Sir W. TURNER, F.R.S. Received January 14,—Read January 24, 1889.

When the lumbar vertebræ of a native Australian, or of several other low races of man, are placed in apposition, the centra form a curved column, with the concavity directed to the front. In other words, the bodies of the lumbar vertebræ are not moulded as in the European, but are wedge-shaped in the opposite direction. This condition can be expressed and contrasted in the different races by formulating a *lumbo-vertebral index*. In calculating this index the anterior vertical diameter of the vertebral body is taken as the standard,

and as equal to 100.* A lumbar vertebra, therefore, with an index of 100, may be regarded as neutral; it is equally deep in front and behind, and can in no way contribute to the formation of a curve in the antero-posterior direction. A vertebra, on the other hand, with an index of 100+, is shaped in a fashion unfavourable to the formation of a curve with the convexity directed forwards; its posterior vertical depth is greater than its anterior vertical depth. Again, a vertebra with an index of 100− is moulded in a manner favourable to the formation of a curve with the convexity looking forwards. It is deeper in front than behind.

In seventy-six European spines, and in seventeen spines of aboriginal Australians, the average indices obtained for the several lumbar vertebræ were as follows:—

Lumbo-vertebral Index.

	76 Europeans.	17 Australians.
Lumbar vertebra I.......	106·1	119·8
" II.......	101·4	113·0
" III.......	97·2	113·6
" IV.......	93·5	103·9
" V.......	81·6	90·4
Lumbo-vertebral index.....	95·8	107·8

The difference brought out by these figures is very marked. Indeed, in this respect the Europeans and Australians constitute the two extremes: no race shows an index lower than that of the European, and no race presents an index higher than that of the Australian.

In the investigations which I made three years ago into the constitution of the lumbar curve in Man and the Apes, I was very early convinced that little could be learned regarding the character and degree of the curve from the lumbo-vertebral index. I was led to adopt this conclusion as the following facts became apparent:—

1. In European spines a high index is not unfrequently associated with a high degree of curvature.

2. In the Chimpanzee, in which the lumbo-vertebral index is so high

* 'Cunningham Memoir,' No. 2, Royal Irish Academy.—"The Lumbar Curve in Man and the Apes," by D. J. Cunningham, M.D. 'Zoology of the Voyage of H.M.S. "Challenger," Part XLVII.'—Sir Wm. Turner's "Report on the Human Skeletons, Part II," p. 67.

as 117·5, the prominence of the lumbar curve exceeds that found in the European spine.

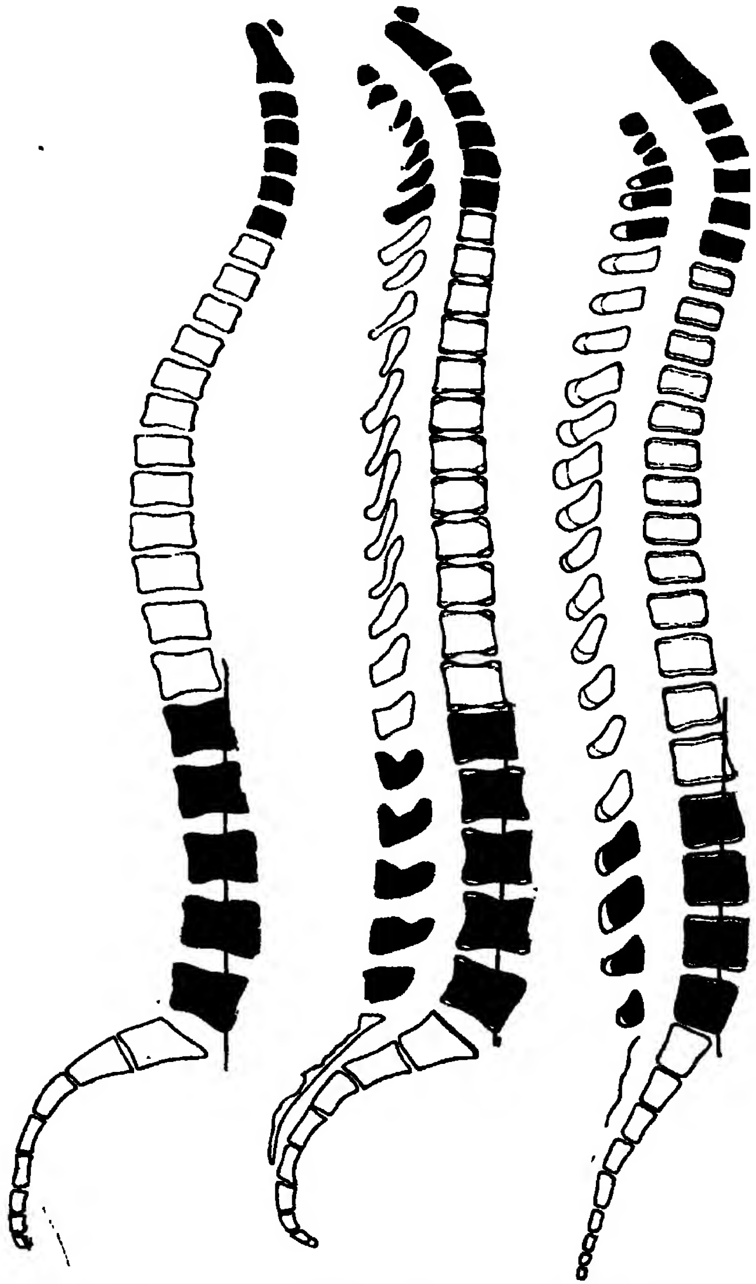
3. In living Bushmen the lumbo-sacral "*ensellure*" is much more marked than in the European.

My views upon this point were expressed as follows:—"From the differences exhibited by the lumbo-vertebral index, some might be inclined to argue that the European had assumed the erect attitude at a period antecedent to the low races. Such a deduction would be altogether untenable. The difference in the form-adaptation of the lumbar bodies with reference to the curve in a European and in a low race can easily be explained when we reflect upon the difference in their habits. The European, who leads a life which rarely necessitates his forsaking the erect attitude except as an intermittent occurrence, and then for short periods, has sacrificed in the lumbar part of the vertebral column *flexibility* for *stability*. It is evident that the deeper the bodies of the vertebræ grow in front the more permanent, stable, and fixed the lumbar curve will become, and the more restricted will be the power of bending forwards at this region of the spine. The savage in whose daily life agility and suppleness of body are of so great an account, who is frequently called upon to pursue game in a prone position, and climb trees in search of fruit, preserves the pithecoid condition of vertebræ in the lumbar region, and on account of this a superior flexibility of the spine must result. . . . . . There is no reason to suppose that this condition is associated with a smaller degree of curvature in this region."

Still in the absence of fresh spines of the lower races, where the lumbar region, composed of combined vertebral bodies and intervertebral disks, could be examined, proof positive upon the degree and character of the lumbar curve was wanting. For more than three years I have made every endeavour to obtain the vertebral column of a native Australian, Negro, Andaman, or Bushman. Through the kindness of my friend Professor T. P. Anderson Stuart, of Sydney University, I have at last succeeded in securing the spine of an Australian girl, aged sixteen. It was sent in a zinc box, in which it was packed with great skill and care. The curvatures were therefore in no way interfered with, and it arrived in a very perfect condition. The erector muscles had been partially removed, but not to such an extent as to produce any material alteration in the flexures.

The flatness of the dorsal curvature and the strongly marked cervical curve were the points in the Australian spine which on cursory examination chiefly attracted attention. In other respects it was apparently little different from the vertebral column of a European. The strong backward bend of the cervical part was perhaps the most striking feature. The flexibility of this region of the spine and the strength and elasticity of the ligamenta subflava

Fig. 1.



EUROPEAN ♀ AUSTRALIAN ♀ CHIMPANZEE ♀

were very remarkable, and altogether confirmed the view which I had formerly expressed upon this point from an examination of the cervical neural spines of the native Australian.*

In order that precise information regarding the curvatures might be acquired, the spine was thoroughly frozen, and then divided in the mesial plane with a saw. A very successful section was obtained, and when the spine was still in the frozen condition, with all its parts immovably fixed, a tracing was taken of each cut surface, and the intervertebral disks and vertebral bodies were measured. At intervals the spine was returned to the freezing mixture, so as to keep it thoroughly consolidated until all the details required were ascertained.

In the accompanying woodcut the tracing of the spine section, reduced by photography, is represented. Similar tracings taken from the spines of an Irish female, aged thirty-five, and a young female Chimpanzee, reduced to a corresponding size. are placed on either side of the Australian spine for purposes of comparison.

The flatness of the dorsal curve in the Australian is very pronounced, and is more marked than in the Chimpanzee, in which both of the primary curvatures (dorsal and sacral) are notably deficient. It further resembles the Chimpanzee in the high degree of cervical curvature which it exhibits.

Points of Inflexion.—In determining these we have not examined the curvature formed by the anterior face of the vertebral column. The true curvature of the vertebral column is that of its axis, and it is this that we have tested. The central points of the bodies of the vertebræ and of the intervertebral disks were carefully ascertained, and a mean curve was drawn through them. In European spines the point at which each curve gives place to that which succeeds it is very constant, and is not affected, so far as my observations go, by the degree of curvature in the different regions. The cervico-dorsal point of inflexion in the European is situated in the disk between the second and third dorsal vertebræ. The lumbo-dorsal point of inflexion in the female is placed in the body of the twelfth dorsal vertebra, but in the male it is a little lower down. The lumbar curve gives place to the sacral curve at a point in the lumbo-sacral disk.

In the Australian spine a greater portion of the dorsal column is involved in the dorsal curve. Above, this curvature only gives way to the cervical convexity in the disk between the first and second dorsal vertebræ, whilst below it includes the last dorsal vertebra, as in the European male, and the change to the lumbar convexity is effected in the dorso-lumbar disk.

These are points of comparatively trifling importance. The great distinction between the Australian and European spine is found in the

* 'Journal of Anatomy and Physiology,' July, 1886.

manner in which the lumbar convexity gives place to the sacral concavity. In the European this takes place in the lumbo-sacral disk of cartilage, and is sharp and sudden, forming a very decided angle. In the Australian it is so gradual and undecided that in the first instance I sketched the lumbar curve so as to include the first sacral vertebra. Such a curve line, however, falls about 1 mm. behind the central point of the lumbo-sacral disk. It is best therefore to consider that the lumbar curve, as in the European, ends in that disk. But the sacral concavity does not begin at once. If the central points of the lumbo-sacral disk, the first sacral vertebra, and the first sacral disk be joined, it will be found that they lie in a straight line, and that the sacral concavity only begins in the first sacral disk. The close association which is thus established between the first piece of the sacrum and the lumbar column is very largely due to the oblique position which is assumed by the last lumbar vertebra. This is a striking peculiarity, and constitutes perhaps one of the most characteristic features of the Australian spine. In my "Cunningham Memoir" I describe a European spine, in which the first sacral vertebra is actually included in the lumbar curve, but this was brought about in a different manner. It was not due to a shifting of the last lumbar vertebra, but to a shifting of the first sacral element which had separated itself from its neighbours, and thus become associated with the lumbar column.

A glance at the tracing of the Australian spine is sufficient to show that the lumbar column is constructed upon principles which are calculated to render it exceedingly flexible and elastic. When we look at the corresponding region of the European an impression of great stability is conveyed to the mind.

In the Chimpanzee, the cervico-dorsal point of inflexion, as in the European, is placed in the disk between the second and third dorsal vertebræ. In the spine figured in fig. 1 the dorsal and lumbar axial curves meet in the central point of the twelfth dorsal vertebra, but in the other specimens which I have examined, the point of demarcation between these curves corresponded to the central point of the intervertebral disk between the twelfth and thirteenth dorsal vertebræ. The flatter dorsal curve of the Australian therefore involves a greater length of the vertebral column than the corresponding deeper curve in the Chimpanzee and European.

But, further, the lumbar axial curve in the Chimpanzee involves one or two of the sacral vertebræ. In the spine of the female Chimpanzee figured in the text, the first and second sacral vertebræ are included, but in other specimens examined, only the first sacral element falls into the line of the lumbar curve. An important gradation is thus established by the Australian spine, in which the first sacral vertebra has just escaped being included in the axial curve of

the lumbar region, and occupies a place which renders it impossible to associate it either with the curve above or the curve below.

In the Chimpanzee the first sacral vertebra is brought into association with the lumbar region by the slight degree of backward inclination of the sacrum; but another factor also comes into play, although to a much less extent, and that is the oblique position of the last lumbar vertebra.

Let us examine these two factors which exercise so marked an influence in producing this modification of the curvature in the Australian and Chimpanzee spines. The sacro-vertebral angle can be tested in a variety of ways, but the most convenient method is to prolong the axis lines of the last lumbar and first sacral vertebræ, and determine the angle which is formed at the point of intersection. In the spines of five European females the average angle thus obtained was 137° 40′. But I am inclined to think that in typical cases the angle in question is not so open. In the young female figured in the text, it is only 117° 20′; whilst in the Australian it was determined to be 141°. In the young Chimpanzee, on the other hand, it attains a magnitude of 166°. But before coming to a definite decision upon this point, it is well to test the matter in another way. This is rendered necessary by the peculiar position of the last lumbar vertebra in the Australian and also, but to a less degree, in the Chimpanzee. Let us take the axis line of the fourth lumbar vertebra in the human spines, and the third lumbar or corresponding vertebra in the Chimpanzee, and note the angle which is formed by the intersection of this by the axis line of the first sacral vertebra. The angles obtained by this method for the three spines figured are:—

European.	Australian.	Chimpanzee.
114°	120°	156°

So far, then, as it is possible to draw a conclusion from one spine, we may say that the sacral obliquity in the Australian is not so marked as in the European. At the same time it is right to state that I have examined individual European spines which exhibited a sacro-vertebral angle as open if not more so than that of the Australian. In these cases, however, special causes existed for this small degree of sacral obliquity.

The peculiar position occupied by the last lumbar vertebra in the Australian can be rendered evident by ascertaining the angle which is formed by the intersection of its axis line by the axis line of the fourth lumbar vertebra. The following are the angles thus determined:—

European.	Australian.
173°	163°

It is this obliquity of the last lumbar vertebra in the Australian which has the effect of so nearly placing the first sacral vertebra in the line of the axial lumbar curve.

In *Troglodytes* the last lumbar vertebra* is closely associated with the sacrum. This constitutes a striking character of the column when seen in mesial section. At first sight the vertebra in question appears to belong to the sacrum. The intervertebral disk which intervenes is very thin; indeed, in a young specimen, it is not much thicker than those interposed between the sacral elements, and it cannot be compared for a moment with the thick pads between the lumbar vertebræ. This intimacy of relationship is still further borne out by the examination of the macerated skeleton, because it is extremely common to find the last lumbar vertebra either fixed to the sacrum by osseous union, or taking on sacral characters by the assumption of the characteristic sacral alæ or rib elements. Of the twelve skeletons of *Troglodytes*, in which the lumbo-sacral region is mentioned in the catalogue of the Museum of the Royal College of Surgeons in England, sevén are described as presenting this peculiarity.

In the European and in the Australian, the last lumbar vertebra is separated from the sacrum by a thick pad of intervertebral substance. Nevertheless, we occasionally find in the human spine the last lumbar vertebra either fused to the sacrum or developing on one or both sides a sacral ala. Professor Kollman, of Basel, has recently exhibited at the Anatomische Gesellschaft at Würzburg (May, 1888), a series of specimens in which the different gradations of this anomaly were illustrated.† During the last four or five years about seventy-five sacra and last lumbar vertebræ have been examined in the macerated state in the Anatomical Department of Trinity College. From these I have obtained one specimen in which the fusion between the last lumbar and first sacral vertebræ is almost complete, and three fifth lumbar vertebræ with a sacral ala developed on one or both sides. We have little information on this point in so far as the spines of the low races are concerned; but it is somewhat significant that in five Australian and in two Andaman skeletons, Sir William Turner‡ should have recorded the occurrence of three fifth lumbar vertebræ with sacral alæ developed upon their transverse

* The vertebral formula being considered as C_7, D_{13}, L_4, S_5, C_4.

† Since writing the above, I have received a letter from Professor Kollman upon this subject. He informs me that he observed the anomaly in eight out of forty-five specimens which he examined. In three cases the assimilation was complete (*i.e.*, on both sides), and in the remaining five it was confined to one side. His specimens were all derived from European skeletons.

‡ 'Zoology of the Voyage of H.M.S. "Challenger," Part XLVII.' "Report on the Human Skeletons, Part II."

processes. It is possible, therefore, that the tendency to assume sacral characters is more marked in the last lumbar vertebra of the Australian than of the European.*

Lumbar Curve.—A single glance at the tracing obtained from the mesial section of the Australian spine, and which has been reproduced in fig. 1, will be sufficient to dissipate any doubt that may be remaining regarding the presence of a lumbar convexity in the vertebral column of this race. Not only does it exist, but it is present in a very pronounced form. Sir William Turner, who has studied the lumbar vertebræ of several of the low races, has endeavoured to arrive at the proper lumbar curvature by careful articulation of the vertebræ. "The upper border of the superior articular facet of the vertebra below was placed in the same transverse plane as the upper border of the inferior articular facet of the vertebra above."† In the Australian he was led to believe that above the level of the lower border of the fourth lumbar vertebra the lumbar column was faintly concave forwards. At the same time he carefully guards himself by insisting that true and trustworthy evidence upon this point can only be acquired by the actual examination of the fresh spine.

A convenient although a somewhat arbitrary way of determining the degree of prominence in the lumbar region in sectional tracings of the spine is to draw a straight line from the anterior extremity of the lower surface of the last lumbar vertebra to the anterior end of the upper surface of the first lumbar vertebra (*vide* fig. 1). By the eye we can readily judge the amount of projection which lies in front of this line in the different tracings, but for accurate comparison it is advisable to formulate an index. This can be done by taking the length of the lumbar column (measured from the centre of the upper surface of the first lumbar vertebra‡ to the centre of the lower surface of the last lumbar vertebra), as the standard and equivalent to 100, and then comparing it with the distance between the intersecting line and the point of greatest prominence. A high index will indicate a strongly pronounced curve, and a low index a feeble degree of curvature. The indices of the lumbar curve ascertained in this way from the three tracings in fig. 1 are the following :—

European.	Australian.	Chimpanzee.
9·7	9·6	10·0

* In twelve Australian skeletons which I have recently examined in the Museum of the Royal College of Surgeons, England, the condition is only present in one specimen.

† 'Zoology of the Voyage of H.M.S. "Challenger," Part XLVII,' &c.

‡ The vertebra, corresponding to the first lumbar vertebra of Man in the Chimpanzee, is the thirteenth dorsal.

The European and the Australian present to all intents and purposes an equal degree of prominence. The Chimpanzee exceeds them both in this respect. It may be well to state that the index expressed by the spine of the European closely corresponds with what I have found to be the average (9·5) for Irish females. On the other hand, when we consider the long period required for the full development and thorough consolidation of the lumbar curve in the human spine, we are forced to admit that it is highly improbable that the index obtained for the Australian expresses the average degree of curvature for that race. The girl from which it was taken was said to be sixteen years old, and the condition of the epiphyses, &c., afforded abundant evidence that the age had not been overstated. Now Balandin,* who has examined the vertebral column in different subjects at the tenth, twelfth, sixteenth, and twentieth year, assures us that in none of these has he found consolidation of the lumbar curve. He considers that it does not become absolutely stable until adult life. Unfortunately we do not possess a sufficient number of tracings of mesial sections of the young spine to come to a decided opinion upon this point; but in the beautiful drawing which is given by Dr. Symington† of such a section of a girl, aged thirteen, the lumbar curve is very feebly marked. I am inclined to consider, therefore, that further investigation will probably show that the curve index of the Australian girl is slightly below the adult standard. The investigations which I carried out upon living Bushmen, and which are recorded in my "Cunningham Memoir," certainly seemed to indicate that in that race, at any rate, the lumbar curve in the erect attitude is in excess of what we find in the European. Of course the greater flexibility which I believe the spines of the black races possess would tend to exaggerate the curve in the standing posture, and at the same time produce the opposite effect when the spine was relieved from its superincumbent burden.

In the Australian spine the point of greatest projection in front of the intersecting line which we have used to determine the degree of lumbar prominence is the anterior border of the upper surface of the fourth lumbar vertebra. This corresponds with what we find in the European male, but in the European female the most projecting point is placed higher, and is formed by the anterior border of the lower surface of the third lumbar vertebra. But the true summit of the lumbar curve is the point of maximum axial curvature, and in the Australian this is situated in the centre of the fourth lumbar vertebra. Again this is identical with what we observe in the European male,

* "Beiträge über die Entstehung der physiologischen Krümmung der Wirbelsäule beim Menschen." Virchow's 'Archiv für Patholog. Anat. und Physiol.,' vol. 57, 1873.

† 'The Anatomy of the Child,' Edinburgh.

but in the European female the summit of the axial curve is placed in the disk, between the third and fourth lumbar vertebræ.

In two particulars, then, the Australian spine resembles the vertebral column of a European male more than that of a European female, viz., in the point of maximum curvature in the lumbar region, and in the fact that the curvature does not include the last dorsal vertebra. The youth of the girl from which it was taken may account for these peculiarities: the spine may not have had time to acquire its special sexual characters, or its full degree of lumbar curvature.

It is self-evident that when a curve is established in a region where the vertebral bodies are not moulded in accordance with it, the production of the curvature must be due to the shape of the intervertebral disks. The difference in height between the anterior and posterior surfaces of the European lumbar vertebræ (with the exception of the fifth) is so slight that it can have little influence in determining the curve in this region. The difference is to be regarded as the *consequence* and not as the *cause* of the curve. The lumbar convexity is mainly produced by the intervertebral disks, and when we reflect upon the manner in which the curvature is called into existence,* we can readily understand why this should be so. In the Australian spine the lumbar prominence is produced entirely by the intervertebral disks. The vertebral bodies, with the single exception of the fifth, are fashioned in a manner unfavourable to the production of a curve of which the convexity is directed forwards.

The table (pp. 498–9) gives the proportions and indices of the lumbar vertebræ and intervertebral disks in the Australian and European spines figured in the text, and also for purposes of comparison the indices of the same parts in four additional European spines.

The index of the lumbar vertebræ in the Australian girl (101·4) is low when we consider that for this race the average index is 107·8. But this average has been largely obtained from males, and there is every reason to believe that the index of the female is very considerably below this. The four female Australian spines which I measured when preparing my "Cunningham Memoir," gave an average lumbo-vertebral index of 103·1, and of these one had an index of 100·9, and another an index so low as 96·7. To all intents and purposes, therefore, the bodies of the lumbar vertebræ in the spine of the Australian girl are neutral in so far as the production of a lumbar curve is concerned. The intervertebral disks are the parts which determine the curvature, and in conformity with this they present the low index of 49·5. A very special feature in this spine is the small amount of depth exhibited by the lumbar disks posteriorly. It is a character which at once appeals to the eye when the tracing of the mesial section is examined. The two disks which contribute most largely

* 'Cunningham Memoir,' No. 2, Roy Irish Acad., p. 78.

Indices of the Vertebral Bodies and Intervertebral Disks of the Lumbar Column.

Australian Spine.

Vertebral bodies.	Actual depth in millimetres Front.	Back.	Index.	Inter-vertebral disks.	Actual depth in millimetres. Front.	Back.	Index.
I	23·0	25	108·2	I	4·5	3·5	77·7
II	24·0	25	104·1	II	6·0	3·0	50·0
III......	25·0	26	104·0	III......	8·5	4·5	53·0
IV	26·5	27	101·8	IV	14·0	4·0	28·6
V	27·0	24	88·8	V	13·0	5·0	38·4
			101·4				49·5

European Spine (fig. 1).

Vertebral bodies.	Actual depth in millimetres. Front.	Back.	Index.	Inter-vertebral disks.	Actual depth in millimetres. Front.	Back.	Index.
I	24·0	23·5	97·9	I	8	7	87·5
II	24·5	24·5	100·0	II	10	9	90·0
III......	25·0	23·0	92·0	III......	11	8	72·7
IV	26·0	23·0	88·4	IV	10	9	90·0
V	26·0	20·0	76·9	V	13	8	61·5
			91·0				80·3

Four Irish Spines (two Males and two Females).

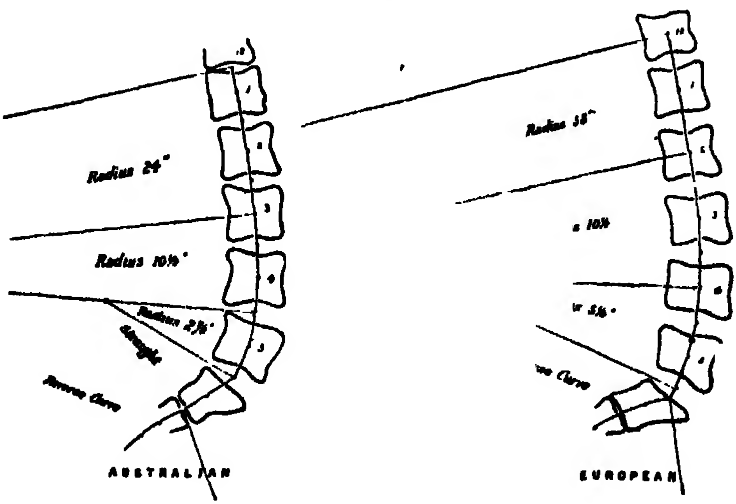
Vertebral bodies.	Index.	Intervertebral disks.	Index.
I	107·7	I	65·2
II	100·5	II	80·0
III................	95·2	III....................	68·0
IV	94·4	IV....	78·2
V	83·7	V	37·0
	96·3		65·6

In this table the indices of the intervertebral disks have been calculated in the same manner as those of the vertebral bodies. The anterior vertical depth in each case has been taken as the standard, and equal to 100. By multiplying the posterior vertical diameter by 100 and dividing the result by the anterior vertical diameter, the indices have been arrived at.

to the curve are the fourth and fifth. The indices of these are 28·6 and 38·4 respectively.

The European spine which we have selected for comparison with the Australian spine also presents a somewhat low lumbo-vertebral index, viz., 91·0. The intervertebral index is consequently higher

Fig. 2.



than is usually the case. Perhaps the average which is given in the table for the four Irish spines more nearly expresses the relative share taken by vertebral bodies and intervertebral disks in the production of the curve. In these the average lumbo-vertebral index is 96·3 and the average intervertebral index 65·6.

In analysing the composition of the axial curve in the lumbar region of the Australian spine, I have had the advantage of the advice and assistance of my friend and colleague Professor Alexander, of the Engineering School in Trinity College, Dublin. The drawings which illustrate this point were executed by him. The axial lumbar curve was found to be composed of the segments of three circles. Thus the portion of the curve which traverses the central points of the fifth lumbar vertebra and of the disk immediately above and below it, constitutes one arc; the central points of the third and fourth lumbar vertebræ, and of the third and fourth intervertebral disks, are traversed by the arc of a second and larger circle; whilst the line passing through the central points of the dorso-lumbar disk and of the first, second, and third lumbar vertebræ with the intervening disks, forms the segment of a third and still larger circle. Segments of three circles can also be detected in the axial lumbar curve of the European, but the parts entering into the formation of these are different. The fourth and fifth lumbar vertebræ with the fourth and fifth disks are ranged in the arc of the lowest and smallest circle; the second, third, and fourth lumbar vertebræ with the two intervening disks, constitute the segment of another circle; whilst the twelfth dorsal vertebra and the first and second lumbar vertebræ, with the interposed disks, form the segment of a third circle.

As I have explained in my "Cunningham Memoir," where I have entered somewhat fully into this point, the composition of the different arcs of the axial curve is one into which many fallacies may creep. Slight inaccuracies in the tracing or a deviation from the mesial plane in sawing the spine will tend to vitiate the results.

The radii of the three arcs which build up the axial lumbar curve present very different lengths. In the lower part of the lumbar column of the Australian the bend is much sharper and more sudden than in the European. This is rendered evident when the radii of the two lowest arcs are compared with each other. Again, the highest segment in the European shows very little deviation from a straight line. It presents a radius more than twice the length of the highest segment in the Australian spine. In the European the lumbar curve is more uniform and gradual throughout. The lengths of the radii of the different arcs of the two spines were ascertained to be as follows:—

	Australian.	European.
Lowest arc	2½ in.	5½ in.
Intermediate arc	10¼ „	10¼ „
Highest arc..............	24 „	58 „

The interposition of a straight piece between the lumbar and sacral curves of the Australian spine is well seen in fig. 2. It certainly offers in this respect a marked contrast to the European, in which the sacral curve breaks off at once from the lumbar curve.

Relative Lengths of the different Regions of the Vertebral Column of the Australian.—Aeby* has called attention to the fact that in the two sexes, and at different periods of life, remarkable differences are found in the relative lengths of the different regions of the vertebral column. The method which we have adopted for ascertaining the degree and quality of the spinal curvature affords us at the same time a very accurate means of comparing the Australian with the European from this point of view. The measurements were made along the fore surface of the spine, and the results obtained showed that the Australian spine corresponds in this respect in the closest manner with the vertebral column of the adult European female. Its total length from the base of the sacrum to the tip of the odontoid process was ·526 mm. The cervical region measured 112 mm., the dorsal region 241 mm., and the lumbar region 173 mm.

In order that we may compare the relative length of each of these regions with what we find in the European, let us regard the fore surface of the movable column as being equal to 100. The following table shows the close similarity which exists in this respect between the Australian girl and the adult European female :—

Fore Surface of Spine from tip of Odontoid Process to Base of Sacrum = 100.

	Australian girl, aged 16.	Average of five adult European females, spines measured in the frozen condition.	16 years old girl (European). (According to Aeby.)
Cervical region	21·3	21·6	21·6
Dorsal region	45·8	45·8	46·9
Lumbar region	32·9	32·8	31·6

* "Die Altersverschiedenheiten der menschlichen Wirbelsäule." 'Archiv für Anat. und Entwick.' 1879.

In the above table, I have also introduced the proportions which Aeby gives for the sixteen-year-old girl. It is curious that the Australian girl should approximate more nearly the adult European female.

Proportion of Bone and Cartilage in the Lumbar Region of the Spine.—When the tracings which are reproduced in fig. 1 are closely examined, it becomes apparent that in the lumbar region the constituent elements, viz., the bones and the intervertebral disks, are not present in corresponding proportions in the different spines. The vertical diameters of the bodies of the lumbar vertebræ differ very appreciably in the three spines, and with this there is a difference in the thickness of the intervertebral disks. In order that we may the more easily contrast the spines from this point of view, I have formulated an index which may be termed the *Sagitto-vertical lumbar index.* In calculating this, the sagittal diameter measured from the centre of the posterior face to the centre of the anterior face of the vertebral body is taken as the standard and equal to 100. The proportion between this diameter and the vertical diameter, measured from the centre of the upper surface to the centre of the lower surface of the vertebral body, can thus be readily expressed. A high index will indicate a long vertebral body; a low index, on the other hand, will indicate a short vertebral body. In the table which follows I have introduced the Baboon, Macaque, and Orang, with the view of enabling us to decide whether or not the difference exhibited in the sagitto-vertical lumbar index of the European, Australian, and Chimpanzee is one from which any important deduction may be drawn.

Two points are rendered very manifest by the above figures, viz., (1), that there is a rapid and decided increase in the length of the lumbar vertebral bodies as we pass from the European, through the Australian, Chimpanzee, and Baboon to the Macaque; and (2), that as the bones elongate the cartilaginous disks become shortened.

The difference in relative length of the lumbar vertebræ in the European and Australian is very marked, the sagitto-vertical index of the former being 80·9, and of the latter 87·0. It must be borne in mind, however, that I have only had an opportunity of examining the one Australian.* Again, it is remarkable that the Orang in the height of its vertebræ should show such a decided deviation from other Apes, and approach so closely to Man. A mesial section through the Orang renders this character apparent to the eye.†

In estimating the vertical depth of both vertebral bodies and inter-

* There are striking sexual differences in this respect. In the male, the bodies of the vertebræ are more compressed. Eight skeletons of female Andaman Islanders afforded a sagitto-vertical lumbar index of 90·4.

† 'Cunningham Memoir,' No. 2, Plate III, Royal Irish Academy.

Sagitto-vertical Index of the Lower Five Movable Vertebræ: also the Proportion of Bone and Cartilage in the corresponding Section of the Vertebral Column.

		Four European females.	Australian girl.	Three Chimpanzees.	One Baboon.	One Macacus nemestrinus.	One Macacus rhesus.	One Orang.
Five lower movable vertebræ..........	I	89·7	82·3	90·5	85·7	113 0	141·1	73·3
	II	89·4	95·6	89·9	95·6	126·6	126·3	89·6
	III........	77·1	88·0	89·8	100·0	126·6	144·4	83·3
	IV........	74·1	88·0	85·4	104·5	126·6	119·0	77·4
	V	74·2	82·1	100·2	109·5	117·4	100·0	86·2
General index............		80·9	87·0	91 2	99·1	122·0	126·2	81·9
Proportion of bone and cartilage in the lumbar column. Lumbar column measured along its axial line = 100.	Bone	64·3	69·4	76·1	80·7	81·0	83·4	73·0
	Cartilage ..	35·7	30·6	23·9	19·3	19·0	16·6	27·0

In calculating the sagitto-vertical index the sagittal diameter of the vertebral body is taken as the standard and equal to 100. By multiplying the vertical diameter by 100 and dividing the result by the sagittal diameter, the index is obtained. The measurements were made from the central point of the anterior face of the vertebral body to the central point of its posterior surface for the sagittal diameter, and from the central point of the upper surface to the central point of the lower surface for the vertical diameter; the higher the index, therefore, the longer is the vertebral body.

vertebral disks, the measurements were made along the axial line of the column. The spines were divided in the mesial plane when thoroughly frozen, so that there was no reduction in the depth of the cartilaginous disks through the bulging out of their central soft portions. In the European we find the largest proportion of cartilage in the construction of the lumbar region. In four female spines the average was found to be 35·7 per cent. cartilage to 64·3 per cent. bone.* In the Australian the amount of cartilage is reduced in conformity with the lengthening of the vertebral bodies; the proportion is 30·6 per cent. cartilage to 69·4 per cent. bone. In the Apes, a still further reduction in the amount of cartilage is manifested; even in the Orang with vertebræ proportionally as short as those of a European, the amount of cartilage in the lumbar part of the spine is relatively much less, viz., in the European 35·7 per cent., and in the Orang 27 per cent. In the Chimpanzee, the marked fall in the amount of cartilage is in a measure due to the extremely thin disk which intervenes between the last lumbar vertebra and the base of the sacrum.

In the erect attitude of Man the greater amount of cartilage lessens the shocks transmitted upwards through the column. In the prone or semi-prone position of the trunk the same provision is not so necessary.

"The Principles of training Rivers through Tidal Estuaries, as illustrated by Investigations into the Methods of improving the Navigation Channels of the Estuary of the Seine." By LEVESON FRANCIS VERNON-HARCOURT, M.A., M.Inst.C.E. Communicated by A. G. VERNON-HARCOURT, F.R.S. Received January 19,—Read February 7, 1889.

(PLATES 2—4.)

The conditions affecting the training of rivers in the non-tidal portions of their course by jetties, or rubble embankments designated as training walls, are well understood. Training walls substitute a straightened uniform channel for irregularities and varying widths, improving the flow of the current and rendering it uniform, so that scour occurs in the shallow, narrowed portions, and more uniformity

* Aeby gives the proportion of bone and cartilage in the different regions of the European spine at different ages, but as he measured the *front aspect only* of the vertebræ and disks, his results cannot be compared with the above. In front and behind the vertical diameters of the disks and vertebral bodies are modified by the spinal curvatures. To obtain the most accurate information regarding the relative proportion of bone and cartilage in a region, the different elements should undoubtedly be measured along the axial curve.

of depth is attained. In very winding rivers, the additional precaution has to be taken of somewhat reducing the width where the deepest channel shifts over from the concave bank on one side to the concave bank on the opposite side at the next bend lower down, so as to reduce the shoal which is found near the point of contrary flexure by concentrating the current at this place.

The training of the outlets of sediment-bearing rivers into tideless seas is determined by the same principles; for a definite discharge is directed and concentrated between training walls or piers, so as to scour a channel across the bar formed, in front of the outlet, by the accumulation of deposit dropped by the enfeebled issuing current. The increased velocity of the current through the contracted outlet carries the silt into deeper water, where it is either borne away by any littoral current, or again forms a bar, after a lapse of time depending on the depth, which can be removed by an extension of the training works.

The training also of the upper part of the tidal portion of rivers has been effected on similar principles to the non-tidal portion, with satisfactory results, even though the problem is, in this case, complicated by the changes in the direction of the current, and the requisite maintenance of the tidal capacity.

In the lower parts, however, of tidal rivers, where the tidal flow predominates, it is difficult to determine the proper width for a trained channel, which, whilst narrow enough to secure an adequate depth, should not very materially check the tidal flow to the detriment of the outlet. Moreover, where the estuary is large, considerable doubt may exist as to the best direction for the training walls; and the establishment of training walls in a wide estuary, where the flood tide is charged with silt, has resulted in extensive accretions,* and corresponding reduction of tidal capacity, by the concentration of the tidal flow and ebb in the trained channel, and a consequent enfeeblement of the currents at the sides, favouring deposit. The principles, indeed, upon which the training of tidal rivers should be based, are in a very undefined and unsatisfactory condition, as exemplified by the conflicting opinions of engineers whenever important training works through estuaries are proposed, as exhibited with reference to the schemes for training works in the upper estuary of the Mersey,† for which the Manchester Ship Canal promoters sought powers in 1883 and 1884, and as at present exist about the extension of the training works in the Ribble estuary.‡ This is due to the various conditions

* 'Instit. Civ. Engin. Proc.,' vol. 84, pp. 246 and 295, and Plates 4 and 5.

† Evidence before Select Committees of Lords and Commons on the Manchester Ship Canal Bills, Sessions 1883 and 1884, and 'Instit. Civ. Engin. Proc.,' vol. 84, p. 309, fig. 7.

‡ 'Instit. Civ. Engin. Proc.,' vol. 84, p. 260, fig. 1.

involved, which differ more or less in each case, and thus render it difficult to lay down general rules for guidance from arguments based on analogy. One of the most important considerations is the form of the estuary; and in this respect no two estuaries are alike, as their form is the result of complex geological and hydrological conditions; and it suffices to contrast the Mersey and the Ribble, the Dee and the Tay, the Clyde and the Tees, the Seine and the Loire, to indicate the varieties of forms which may have to be dealt with. Other circumstances affecting the problem are the rise of tide, the tidal capacity and general depth, the fresh-water discharge, the silt introduced by the flood tide or brought down by the river, the condition of the sea bottom in front of the mouth, and the direction in which the tidal current enters the estuary. The positions also of ports established at the sides of estuaries require special consideration in determining the proper line for a trained channel. These numerous and variable conditions have often led engineers to ennunciate the opinion that each river must be considered independently by itself. This view, however, if strictly adhered to, by excluding the experience derived from previous works, would prevent any progress in the determination of general principles for the improvement of navigation channels through estuaries; each training work would form an independent scheme, based upon no previous experience, and might or might not produce the results anticipated by its designer. Unfortunately also it is impossible to proceed with training works by the method of trial and error; for besides the cost of modifying the lines of training walls, if the desired results are not produced, these works generally effect such extensive changes in an estuary, that it would be impracticable to restore the original conditions, or to modify materially the altered position.

It might be possible to deduce general rules for training works from a careful consideration of a variety of types of estuaries, especially those in which training works have been carried out; and I have commenced an investigation of this kind. This method of inquiry, however, requires a variety of data which it is difficult to obtain for most estuaries, and must depend upon a careful estimate of the relative influence of each of the variable conditions, and a train of reasoning from analogy which might not be accepted by engineers as conclusive. Accordingly, it would be of the very highest value to river engineers, and of considerable interest from a scientific point of view, if a method of investigation could be devised, which might be applied to the special conditions of any estuary, and the results of any scheme of training works determined approximately beforehand, in a manner which could be relied upon from the fact of their depending on an assimilation to the actual conditions of the case investigated, and not on arguments based upon the effects of similar works under

more or less different conditions. The following description is therefore given of the results of investigations, carried on at intervals during more than two years, with reference to the proposed extensions of the training works in the Seine estuary, which appear to afford a fair assurance that a similar method, applied to any estuary, would indicate the effect of any scheme of training works, provided the special conditions of the estuary were known.

Investigations about the Seine Estuary.

The training works in the lower portion of the tidal Seine, commenced in 1848, had reached Berville in 1870, when the works were stopped, in the interests of the port of Havre, on account of the large unexpected accretions which were taking place behind the training walls, and at the sides of the wide estuary below them.* The original scheme, proposed in 1845 by M. Bouniceau,† comprised the extension of the trained channel to Honfleur on the southern side of the estuary, and the prolongation of one or both of the training walls towards Havre at the north-western extremity of the estuary, as in any scheme, the interests of both these ports, on opposite sides of the estuary, have to be considered. The works are acknowledged to be incomplete; and great interest has been evinced, particularly within the last few years, in the question of their extension, so that the shifting channel between Berville and the sea may be trained and deepened, and the access to Honfleur improved, without endangering the approaches to Havre. The objects desired are distinctly defined; but the means for attaining them have formed the subject of such a variety of schemes, that hardly any part of the estuary below Berville has not been traversed by some proposed trained channel, except the portion lying north of a line between Hoc and Tancarville points, which is too far removed from Honfleur to be admissible for any scheme. Altogether, including distinct modifications, fourteen schemes have been published in France within my knowledge, seven of them having appeared within the last five years. The schemes also exhibit great varieties in their general design ‡ (Plate 2, figs. 1 and 3; Plate 3, figs. 1 and 2; and Plate 4, fig. 1), illustrating very forcibly the great uncertainty which exists, even in a special case where the conditions have been long studied, as to the principles which should be followed in designing training works. It is evident that no reasoning from analogy could prevail amongst such very conflicting views; and having had the subject under consideration for a long time, the idea occurred to me in August, 1886, of attempting the solution of this very difficult problem by an experimental method, which might also throw light upon general

* 'Instit. Civ. Engin. Proc.,' vol. 84, p. 241, and Plates 4 and 5.

† 'Étude sur la Navigation des Rivières à Marées,' M. Bouniceau, p. 152, Plate

‡ 'Instit. Civ. Engin. Proc.,' vol. 84, p. 247, and Plate 4, fig. 9.

principles for guidance in training rivers through estuaries. The estuary of the Seine is in some respects peculiarly well adapted for such an investigation, for old charts exhibit the state of the river before the training works were commenced, and recent charts indicate the changes which the training walls have produced, whilst the various designs for the completion of the works, proposed by experienced engineers, afford an interesting basis for experimental inquiries into the principles of training works in estuaries. If, in the first place, it should be possible to reproduce in a model the shifting channels of the Seine estuary as they formerly existed, and next, after inserting the training walls in the model as they now exist in the estuary, the effects produced by these works could be reproduced on a small scale, it appeared reasonable to assume that the introduction, successively, in the model of the various lines proposed for the extension of the training walls would produce results in the model fairly resembling the effects which the works, if carried out, would actually produce.

When the third Manchester Ship Canal Bill was being considered by Parliament in 1885, Professor Osborne Reynolds constructed a working model of the portion of the Mersey estuary above Liverpool on behalf of the promoters of the canal, with the object of showing that no changes would be produced in the main channels of the estuary by the canal works which had been designed to modify very slightly the line of the Cheshire shore above Eastham. This model was, I believe, the first experimental investigation on an estuary by artificially producing the tidal action of flood and ebb on a small scale; and Professor Reynolds' experiment showed that a remarkably close resemblance to the main tidal channels in the inner estuary could be produced on a small scale.

As the Mersey model did not extend into Liverpool Bay, the tidal action produced was very definitely directed along the confined channel representing the "Narrows" between Liverpool and Birkenhead; and this tidal flow was not perceptibly influenced by the relatively very small fresh-water discharge. In the Seine, however, there is no narrow inlet channel to adjust exactly the set of the flood tide into the estuary; and the large fresh-water discharge of the Seine, with a basin about eighteen times larger than the Mersey basin, forms an important factor in the result. The tide in a model of the Seine has to be produced in the open bay outside the estuary at a suitable angle which had to be determined; and it was essential for the success of the Seine experiments that accretion should be produced in the model of the Seine estuary under certain circumstances, which was a condition which did not enter into the Mersey problem. Accordingly, the very interesting and valuable results obtained by Professor Reynolds, in his model of the Mersey, could afford no assurance that

experiments involving essentially different and novel conditions would lead to any satisfactory results. I therefore restricted the requirements for my experiments within the smallest possible limits, and contented myself with the simplest means, and the limited space available in my office at Westminster.

Description of Model of the Seine Estuary.—The model representing the tidal portion of the River Seine and the adjacent coast of Calvados, extending from Martot, the lowest weir on the Seine, down to about Dives, to the south-west of Trouville, was moulded in Portland cement by my assistant, Mr. Edward Blundell, to the scales of $\frac{1}{40000}$ horizontal and $\frac{1}{400}$ vertical. The first is the scale of some of the more recent published charts of the Seine—and even at that scale the model is nearly 9 feet long; whilst I made the vertical scale one hundred times the horizontal, as the fall of the bed of the tidal Seine is very slight, and the rise of spring tides at the mouth, being 23 feet 7 inches, amounted to an elevation of the water in the model of only 0·71 inch. There are two banks at the mouth of the estuary, between Havre and Villerville Point, known as the Amfard and Ratier banks, which emerge between half-tide and low water, and divide the entrance to the estuary into three channels. Through all the changes in the navigable channel at the outlet, these banks always appear in some form or other in the low-water charts, either connected with the sandbanks inside the estuary, or detached. On examining the large chart drawn from the survey made by M. Germain in 1880, I found that rock and gravel cropped up to the surface over a certain area on these banks, and accordingly I introduced solid mounds at these places to represent the hard portions of the Amfard and Ratier banks, which are permanent features in the estuary. As a rocky bottom is found near Havre, and also at Villerville Point on the opposite side of the outlet, Amfard and Ratier banks are doubtless the remains of a rocky barrier which in remote ages stretched right across the present mouth of the river. Where the rocky bottom lies bare near Havre and Villerville, the model was moulded to the exact depths shown on the chart of 1880; but in other places the cement bottom was merely kept well below the greatest depth the channel had attained at each place, whilst the actual bed of the estuary in the model was formed by the flow of water over a layer of sand.

Arrangements for Tidal and Fresh-water Flow.—The mouth of the Seine estuary faces west; but the tidal wave comes in from the north-west, and the earliest and strongest flood tide flows through the northern channel between Havre and the Amfard bank; whilst the influx through the southern Villerville channel occurs later, and is stronger towards high water. Accordingly, the tidal flow had to be introduced from a northerly direction, at an angle to the mouth of the estuary; and the line of junction of the hinged tray, producing the

tidal rise and fall, was made at an angle of about 50° to a line running from east to west in the model, so that the tidal flow approached the estuary from a point only about 5° to the west of north-west. The tray was made of zinc, enclosed by strips on three sides to the height of the sides of the estuary; and it was hinged to the model, at its open end, by a strip of india-rubber sheeting along the bottom and sides, so as to make a water-tight joint with sufficient play at the sides to admit of the tray being tipped up and down from its outer end. The rise and fall of the tray was effected by the screw of a letter press, from which the lower portion had been detached, by raising and lowering the upper plate of the press, half of which was inserted under the tray. After the requisite amount of sand had been introduced to raise the bottom to the average level, the model was filled with just enough water for the surface of the water to represent low water of spring tides when the tray was down and the screw at its lowest limit; and the tray was made of such a size that, when the screw was raised to its full extent, the water in the model was raised, by the tipping of the tray, to the level representing high water of spring tides. The water representing the fresh-water discharge of the Seine was admitted into the upper end of the model from a tap in a small tin cistern; and the efflux of a similar quantity of water was provided for at the lower extremity of the estuary, on its northern side near the tray, by a cock with a larger orifice placed at such a level as to allow the water to flow out into a second cistern, of similar size, during the higher half of the tide.

First Results of Working the Model.—The construction of the model was commenced in October, 1886, and its working was commenced in November. Though the Portland cement was convenient for moulding in a small space and in the absence of appliances, it did not prove satisfactory for retaining water at first. The model was purposely made in two halves, and the straight joint was subsequently made water-tight; but, nevertheless, cracks occurred at various places through which the water leaked, and they had to be repaired as they appeared; and the bottom of the model was eventually coated with thick varnish, and after a time the leaks ceased. The flexible india-rubber hinge, from which I had anticipated some trouble, leaked very little from the beginning, and on being fitted with greater care in introducing a tray of somewhat different form, no leakage occurred.

Silver sand was used in the first instance for forming the bed of the estuary. From the outset, the *bore* at Caudebec, indicated by a sudden rise of the water, and the reverse current just before high water near Havre, called the "*verhaule*," were very well marked. The *verhaule* is evidently a sort of back eddy, on the northern shore, occasioned by the influx of the tide, and by the final filling of the estuary from the southern channel; whilst the *bore* appears to

result from the concentration of the tidal rise by the sudden contraction of the estuary above Quillebeuf. The period given to each tide in working was about 25 seconds, which appeared fairly to reproduce the conditions of the estuary.* After the model had been worked for a little time, the channels near Quillebeuf assumed lines resembling those which previously existed; and a small channel appeared on the northern shore, by Harfleur and Hoc Point, which is clearly defined in the chart of 1834. The main channel also shifted about in the estuary, and tended to break up into two or three shallow channels near the meridian of Berville, where the influences of the flood and ebb tides were nearly balanced. The model, accordingly, fairly reproduced the conditions of the actual estuary previous to the commence- of the training walls; though the channel in the estuary did not attain the depth, as represented by the proportionately large vertical scale, which the old channels possessed, owing, doubtless, to the comparatively small scouring influence which the minute currents in the model possess. The sand, in fact, cannot be reduced to a fineness corresponding to the scale of the model, whilst the friction on the bed is not diminished equivalently to the reduction in volume of the current. Silver sand had been used on account of its being readily obtained, its purity, and absence of cohesion, as it was hoped that the water, by percolating freely through it, would more readily shift it. A film, however, seemed by degrees to form over its surface, reducing considerably its mobility; and as the action of the water on it consisted merely in rolling the particles along the bottom, this sand did not prove satisfactory for producing the requisite changes when the training walls were inserted in the model. It became, therefore, essential to search for a substance which the water could to some extent carry in suspension for a short period.

Trial of Various Substances for Forming the Bed of the Estuary.— Some substance was required, not necessarily sand, insoluble in water, easily scoured, and therefore not pasty or sticky, and sufficiently fine or light to be carried in suspension to some extent by the currents in the model, and not merely rolled along the bottom like the silver sand. A variety of substances of low specific gravity, and in powdered form, were accordingly tried in succession during the first half of 1887. Pumice in powder proved too sticky; and flower of sulphur was too greasy to be easily immersed in water. Pounded coke was too dirty to be suitable, and particles of it floated. Violet-powder became too pasty in water; and fuller's earth and lupin seed exhibited similar defects. The grains of coffee grounds were too large in water, and moved up and down in the currents too readily; whilst fine sawdust

* According to the formula in the paper by Professor O. Reynolds, on his Mersey model, read at the Frankfort Congress in August, 1888, the tidal period would be nearly 23 seconds.

from boxwood and lignum vitæ swelled in water, and was carried along so very easily by the stream that no definite channels were formed in it. The powder obtained from Bath brick, which was experimented upon for some time in the model, both without and with training walls, yielded more satisfactory results, as besides affording shifting channels like the silver sand, it accumulated at the sides of the estuary when the training walls were introduced in the model. It, however, gradually became too compact, so that the current could no longer produce much effect on it; but as it is probable that some sticky material is used in the manufacture of Bath bricks, it is quite possible that if I had succeeded in my endeavour to obtain the silt of the River Parret, from which the bricks are made, in its natural state, the material might have proved more subject to scouring influence.

At last, in July 1887, I found a fine sand, on Chobham Common, belonging to the Bagshot beds, with a small admixture of peat. This sand, besides containing some very fine particles, was perfectly clean, so that water readily percolated through it; and it accordingly combined the advantages possessed by silver sand with a considerably greater fineness.

Results of Working Model with Bagshot Sand.—The bed of the estuary having been formed with the sand obtained from Chobham Common, after the model had been worked for some time, the channels assumed a form very closely resembling the chart of the Seine estuary of 1834.* Accordingly, the first stage of the investigation was duly accomplished by the reproduction of a former state of the estuary in the model, with the single exception of a decidedly smaller depth in the channels, except in places where the scour was considerable, which is readily accounted for by the circumstances of the case. It is probable that with a larger model, and especially if the bed was not so nearly level as in the Seine, the depth would approach nearer to the proper distorted proportion as compared with the width.

The close correspondence of the channels in the model with an actual state of the estuary in its natural condition, confirms, in a considerably more complicated case, the results previously achieved by Professor Reynolds with reference to the upper estuary of the Mersey, and affords a fair certainty that, with adequate data, the natural condition of any estuary could be reproduced on a small scale in a model.

Introduction of the Existing Training Walls in the Model.—The second stage of the investigation consisted in the introduction of training walls into the model, corresponding in position to the actual training walls established in the estuary down to Berville. These walls, formed with strips of tin, cut to the corresponding heights at the different places, and bent to the proper lines, were gradually

* 'Instit. Civ. Engin. Proc.,' vol. 84, Plate 5, fig. 1.

inserted in sections; and the model was worked between each addition, to conform, as far as practicable, to the actual conditions. The fine particles of the sand accreted behind the training walls; and the channel between the walls was scoured out, corresponding precisely to the changes which have actually occurred in the estuary of the Seine. The foreshores at the back of the training walls were raised up in some parts to high-water level, whilst in other places the accumulation was somewhat retarded by the slight recoil of the water from the vertical sides of the model, and by the wash over the vertical training walls, these forms being necessitated by the great distortion of the vertical scale of the model. On the whole, however, the accretion and scour in the model correspond very fairly to the results produced by the existing training walls in the estuary. The accretion, moreover, in the model, extended beyond the training walls on each side, down to Hoc Point on the right bank, obliterating the inshore channel close to Harfleur, which had been reproduced in the model, and down to Honfleur on the left bank, corresponding in these respects also to the actual changes in the estuary.* The main channel also, beyond the ends of the training walls, was comparatively shallow, and was unstable, reproducing the existing conditions in the estuary.

The experiments relating to this stage extended over a year and a half, taking up all the time that could be spared to them by myself and my assistant during that period; they formed the turning point of the investigation, and have the interest of being, as far as I am aware, the first attempt at putting training walls in a model, and obtaining the resulting accretion on a small scale. Without the accomplishment of this stage, it would have been useless to continue the investigation; and its satisfactory attainment proved so difficult in actual practice, that for a long time it seemed probable that the attempt must be abandoned.

Application of System to Ascertain the Probable Effects of any Training Works.—As the first and second steps in the investigation, by the aid of the model, had furnished results which corresponded very fairly with the actual states of the estuary of the Seine before and after the execution of the training works, the final stage of the investigation, for ascertaining the probable results of any extensions of the training walls, could be reasonably entered upon. In selecting the lines of training walls to be experimented on, it appeared expedient to adopt those which have been designed, after careful study, by experienced engineers, both on account of the results from these being far more interesting than those of a variety of theoretical schemes, and also in the hope that some assistance might thereby be rendered to French engineers in the prosecution of this important

* 'Instit. Civ. Engin. Proc.,' vol. 84; compare Plate 5, fig. 1, and Plate 4, fig. 1.

work. Moreover, the schemes exhibit sufficient variety to admit of their being taken as types of schemes for throwing light upon the principles on which training works should be designed in estuaries. Accordingly, the third stage in the investigation consisted in extending the training walls in the model, in accordance with the lines of some of the schemes proposed; and, after working the model for some time with each of the extensions successively, the several results were recorded, as shown in Plates 2 and 3, and Plate 4, figs. 1 and 2. The lines of training walls experimented on in the model were taken, with one exception, from five out of the seven most recent schemes proposed, as these five schemes are, I believe, the only ones which are still put forward for adoption. The lines shown on Plate 4, fig. 3, represent merely a theoretical arrangement of training walls, inserted for a final experiment in the model, to ascertain the effect of the most gradual enlargement of the trained channel which the physical conditions of the estuary would have admitted of at the outset, whilst maintaining the full width at the mouth.

Scheme A.—The first arrangement of extended training walls introduced into the model was taken from a scheme, some of the main features of which were proposed in an earlier scheme in 1859,* and which was put forward in an amended form in 1886.† The design, as inserted in the model, consisted of an extension of the parallel training walls from Berville down to Honfleur, and the formation of a breakwater across the outlet, from Villerville Point on the southern shore of the estuary, out to the Amfard bank, thus restricting the mouth to the channel between Amfard bank and Havre. The lines of these works were formed in the model with strips of tin, as shown on Plate 2, fig. 1; the northern training wal was kept low, and the southern wall was raised to the level representing high water of neap tides; whilst the strip representing the breakwater was raised above the highest tide level, thus forcing all the flood and ebb water to pass through the Havre Channel. The results obtained in the model with these arrangements, after working it for about 6000 tides, are indicated on the first chart (Plate 2, fig. 1). The channel between the prolonged training walls had a fair depth throughout, partly owing to the concentration of the fresh-water discharge between the walls, and partly from the retention of some additional water in the channel at low water, by the hindrance to its outflow offered by a sandbank which formed in front of the ends of the training walls. A deep hole was soon scoured out in the narrowed outlet by the rapid flow of the water filling and emptying the estuary at every tide. The absence, however, of connexion between the direction of the flood tide current

* 'La Seine comme Voie de Communication Maritime et Fluviale,' J. de Coene, 1883, p. 11, and Plate 7.

† 'Projet des Travaux à faire à l'Embouchure de la Seine,' L. Partiot, Paris, 1886.

through the outlet and the ebbing current from the trained channel, aided by the accretion of sand in the sheltered recess behind the breakwater, led eventually to the formation of two almost rectangular bends in the channel, one just beyond the training walls, and the other near Hoc Point in the model. This tortuous channel, moreover, was shallow, except at the bends and the outlet; and a bar was formed a short distance beyond the outlet. The contraction of the mouth of the estuary by the breakwater interfered so much with the influx of the tide into the estuary as to render it impossible to raise the tide inside to its previous height; and the reduction in height of the tide was clearly marked at Tancarville Point in the model. Sediment accumulated in the estuary beyond the trained channel, being brought in by the rapid flood current, and not readily removed by the ebb, except in the trained channel and near the outlet; and this accretion, by diminishing the tidal capacity, gradually reduced the current through the outlet, and consequently the depth of the outlet channel. A considerable accumulation of sand took place outside the breakwater, along the southern sea-coast, so that the bank opposite Trouville in the model was connected with the shore, and the foreshore advanced towards the end of the breakwater (Plate 2, fig. 1).

Scheme B.—The second arrangement of training walls inserted in the model, below Berville, was taken from a scheme proposed in 1888, representing a modification, by another engineer, of the design from which Scheme A was copied.* It comprised the retention of the breakwater from Villerville Point to the Amfard bank, the most essential feature in Scheme A; but the extension of the northern training wall was dispensed with, whilst the southern training wall was prolonged, in a continuous curve, from Berville to Honfleur (Plate 2, fig. 2), and eventually to the Amfard bank, connecting it there with the extremity of the breakwater (Plate 2, fig. 3). A slight widening out of the existing trained channel, by an alteration of the end portion of the northern training wall, completed the arrangement of the model. The results obtained by inserting the training wall down to Honfleur, and then working the model for about 3500 tides, are shown in Plate 2, fig. 2; and those obtained after the prolongation of the southern training wall to the breakwater, and working the model for about 3700 tides, are shown in Plate 2, fig. 3. The channel followed pretty nearly the concave line of the prolonged southern training wall, between Berville and Honfleur in the model, except near Berville; but the depth of water was less regular than in the previous experiment, owing to the diminished concentration of the ebb from the absence of the northern training wall. The channel

* 'Mémoires de la Société des Ingénieurs Civils,' Mars, 1888, Paris, pp. 257 and 278, and Plate 162, fig. 2.

between Honfleur and Amfard was tortuous as before, but its direction was different. The deep hole at the outlet, the bar beyond, and the advance of the southern foreshore beyond the breakwater, reappeared again with very similar features to those in the first scheme, except that the sandbank did not quite reach the outside face of the breakwater at low water. (Compare fig. 2 with fig. 1 in Plate 2.)

The results which followed from working the model with the southern training wall prolonged to Amfard, are shown in Plate 2, fig. 3. The main alteration from the former experiment naturally occurred between Honfleur and Amfard in the model, a continuous channel being formed along the new piece of concave training wall; whilst the general depth inside the estuary was improved as far as the meridian of Hoc Point. The channel, however, above Honfleur was not improved, owing apparently to the want of uniformity between the directions of the flood and ebb currents in the model. The other features remained very similar to the former case, except that the end of the sandbank beyond the breakwater was slightly eroded, whilst deposit took place between the extended training wall and the breakwater. (Compare fig. 3 with fig. 2 in Plate 2.)

Scheme C.—The third arrangement of training walls experimented upon in the model was chosen from a design published in 1885.* It consisted of an enlargement of the original trained channel below Quillebeuf, by a modification of the southern training wall from Quillebeuf, and of the northern training wall from Tancarville, and the extension of the northern wall to Amfard and Havre, and the southern training wall to Ratier, as shown on Plate 3, fig. 1. The trained channel was thus given a curved, gradually enlarging form, and was directed into the central channel of the model, between Ratier and Amfard, the Villerville and Havre channels being practically closed near low water. The effects of working the model for about 6500 tides with this arrangement of training walls are indicated on the chart (Plate 3, fig. 1). The main channel kept near the concave southern training wall for some distance below Berville, and then gradually assumed a more central course between the training walls towards the outlet, passing out just to the south of the Amfard bank. The channel thus formed had a good, tolerably uniform depth, together with a fair width, owing apparently to the flood and ebb tides produced in the model following an unimpeded and fairly similar course. Deposit occurred behind the training walls on each side; and the foreshore advanced in front of Trouville in the model, in consequence of the shutting up of the Villerville Channel.

Scheme D.—The fourth arrangement of training walls adopted in

* 'La Seine Maritime et son Estuaire,' E. Lavoinne, Paris, 1885, p. 140, and 'Instit. Civ. Engin. Proc.,' vol. 84, p. 248, and Plate 4, fig. 9.

the model was selected from the most recent design* proposed by an engineer who had previously submitted schemes in 1881† and 1886.‡ The trained channel was widened out by an alteration of the southern wall from Quillebeuf, and the northern wall from Tancarville, more than trebling the width between the training walls at Berville in the model; and the walls were extended in sinuous lines to Havre on the northern side, and Honfleur on the southern side, as shown on Plate 3, fig. 2, thus forming a winding trained channel rapidly enlarging near its outlet. The model, with these lines of training walls, was worked for about 5000 tides, with the results indicated on the chart. Deep channels were scoured out close along the inner concave faces of the training walls in the model; but shoals appeared over a considerable area of the newly trained channel; a bar stretched across the deep channel where it shifted over from the south to the north training wall, about half way between Berville and Honfleur; and a large sandbank, emerging above low water, occupied the centre of the outlet opposite Honfleur. Deposit also occurred at the sides of the estuary behind the training walls.

As it was of importance to ascertain to what extent accidental modifications in the arrangement of the sand in the preparation for an experiment might affect the result, the lines of training walls described above were inserted a second time in the model, after the subsequent scheme E had been experimented upon, rendering it necessary to replace afresh both training walls, and to remodel the sand so as to represent approximately the present condition of the estuary. The model was prepared for this second experiment in the usual way, without any special endeavour to secure coincidence with the first experiment in the initial arrangement of sandbanks and channels. The condition of the low-water channels in the model, after working the model with this arrangement of training walls for the second time for about 5400 tides, is shown on Plate 3, fig. 3. The main features of the trained channel in the charts of the two experiments exhibit a very fair resemblance, considering the modifications which any alterations in the initial condition might produce, and the naturally variable state of the channels in a wide outlet. The deep channels reappear in the second chart at the inner concave faces of the training walls, with intervening shoals; a large sandbank is again visible at low water along the north training wall, opposite La Roque and Berville in the model; and the sandbank in the centre

* 'Déposition de M. Vauthier devant la Commission des Ports et Voies Navigables de la Chambre des Députés,' Paris, 1888, p. 17, and Plate 4.

† 'Rapport sur les Améliorations dont sont encore susceptibles la Seine Maritime et son Estuaire,' L. L. Vauthier, Rouen, 1881, p. 46, and Annex 29.

‡ 'Dire à l'Enquête ouverte sur l'Avant-projet des Travaux d'Amélioration de la Basse-Seine, 1886,' L. L. Vauthier, Paris, Plate 1.

of the outlet of the trained channel opposite Honfleur emerges again, though smaller in extent owing to alterations in the channel; and the deep place at the end of the southern training wall, close to Honfleur, is the same in both charts.

Scheme E:—The fifth arrangement of training walls introduced into the model was taken from a design* published in 1888, which is a modification of a scheme, presented in 1886, by a Committee of experts appointed by the French Government to consider the question.† In the scheme as laid down in the model, the trained channel in the bend between Quillebeuf and Tancarville, where the depth was greatest, was enlarged in width by setting back the southern training wall; the original width of the channel was retained at the point of inflexion opposite Tancarville, and the channel was widened out below La Roque by a modification of the lines of both training walls down to Berville. The training walls were also extended beyond Berville in sinuous lines, as shown on Plate 4, fig. 1, the southern wall being carried down to Honfleur, and the northern wall not quite so far. The portion forming the last bend of the northern training wall was kept low, whilst the others were made high, according to the design. Both in this and the preceding arrangement of training wells experimented on, the expanding trained channel was somewhat restricted in width along the portions near the changes of curvature, to make it conform to the principles which experience has laid down for training winding rivers in their non-tidal course, as previously mentioned. The results obtained, after working the model for about 3700 tides, are represented on the chart (Plate 4, fig. 1). The channel between the training walls was somewhat shallow in places; and though a deep channel was formed along the inner concave face of the southern wall between La Roque and Berville, a shoal emerging above low water appeared along the concave face of the last bend of the northern training wall. This bank appeared to be due to the protection the extremity of the bend afforded from the action of the flood tide in the model; whilst the ebb followed the central flood-tide channel, instead of passing over to the concave bank as would have occurred with the current of a non-tidal river. The main channel beyond the training walls, which, though of fair depth, was somewhat narrow and winding, was also unstable; for in the early part of the experiment, its outlet was in the central channel between Ratier and Amfard in

* 'De l'Amélioration du Port du Havre et des Passes de la Basse-Seine,' Baron Quinette de Rochemont, Paris, 1888, excerpt 'Mémoires de la Société des Ingénieurs Civils,' 1888, p. 324, Plate 162, fig. 1.

† 'Commission d'Étude des Améliorations à apporter au Port du Havre et aux Passes de la Basse-Seine,—Rapport de la Commission,' Paris, 1886, p. 61, and chart.

the model, whilst at the close of the experiment it had shifted, as shown, to the Havre channel. Accretion occurred behind the training walls in the model; and some silting up took place in the Villerville Channel and along the foreshore in front of Trouville, owing apparently to the preference of the main channel for the other outlets, and the diminished capacity of the estuary resulting from accretion.

This arrangement of training walls was further investigated by working the model for about 6300 tides more, with the results shown on Plate 4, fig. 2. The chief features of the estuary in the model showed only slight changes from the state previously recorded (Plate 4, fig, 1), with the exception of the main channel which had shifted again to the central outlet; whilst the northern foreshore above low water extended over part of the former site of the channel. The two conditions of the estuary, represented by Plate 4, figs. 1 and 2, have therefore the interest of exhibiting in the model a shifting channel, such as actually exists at the present time in the Seine estuary below Berville.

Scheme F.—The last experiment was made on an arrangement of training walls inserted in the model, making the trained channel expand as gently as practicable between Aizier and the sea, whilst retaining the natural width at the outlet (Plate 4, fig. 3). This is the form of channel which theory indicates as the most suitable;* for whilst it facilitates the influx of the flood tide, it prevents, as far as possible, the abrupt changes in the velocity of a river in passing from its estuary to the sea, which are so prejudicial to uniformity of depth in a channel. It was therefore of interest to ascertain what results would be produced by this theoretical arrangement of training walls in the model, which, in order to leave the outlet free, and thus avoid favouring a progression of the foreshore outside, had to provide a wide channel near Honfleur compared with the restricted width available at Quillebeuf. The direction of the channel between Aizier and Quillebeuf, together with the cliffs bordering the river at Quillebeuf and Tancarville Points, determined the maximum width obtainable at Quillebeuf, and the direction of the channel from Aizier to Tancarville; and the extension of the training walls in the model from this point was regulated by the necessity of passing close to Honfleur at the south, and not impeding the approach to Havre on the north. The effects produced in the model by working with this arrangement of training walls for about 7300 tides are indicated on the chart (Plate 4, fig. 3). The southern training wall was kept above high-water level all the way to its termination at Honfleur in the model, but the northern training wall was gradually reduced in height from nearly opposite Honfleur towards Havre. The trained

* 'Rivers and Canals,' L. F. Vernon-Harcourt, p. 236.

channel had a good width at low water throughout, in spite of the distance apart of the training walls in the model, the whole channel being below low-water level, except near the southern wall between Berville and Havre, and against the northern wall nearly opposite Hoc Point, where banks emerged slightly above low water. The channel, moreover, was distinctly, though slowly, improving with the continuance of the working, and the banks diminishing. There was also a fair depth in the channel, the shallowest place being opposite Berville, whilst a deep place was formed just above, near the southern wall between La Roque and Berville. The depth in all the outlet channels was well maintained; and though deposit naturally took place behind the northern training wall, no accretion was visible along the foreshores outside.

Considerations affecting Experimental Training Works.

The value of experiments resembling those just described depends entirely upon the extent to which they may be regarded as producing effects approximately corresponding, on a small scale, to those which training works on similar lines, if carried out in an estuary, would actually produce. If the effects of any training works could be foreshadowed by experiments in a model, the value of such experiments, in guiding engineers towards the selection of the most suitable design, could not be overestimated.

Some of the influences at work in an estuary cannot possibly be reproduced in a model—such as winds and waves. Winds coming from different quarters are variable in their effects; but the direction of the prevailing wind indicates the line in which the action of the wind has most influence, which may be exerted in reinforcing the flood or ebb currents, and may aid or retard accretion by blowing the silt-bearing stream more into or out of the estuary. Waves are the main agents in the erosion of cliffs along open sea-coasts, and in stirring up sand in shallow places; and the material thus put in suspension may be transported by tidal currents, aided by wind, into an estuary, and be deposited under favourable conditions. These circumstances affect the rate of accretion, which cannot be investigated experimentally, as it is impossible to reproduce in a model the proportion of silt in suspension, which, moreover, varies in any estuary with the state of the weather and tide, and the volume of fresh water discharged. Inside an estuary, also, waves in storms may erode the shores at high tide, and modify the low-water channels; but the first effect is very gradual, and the second is intermittent—only occasionally occurring.

The main forces acting in any tidal estuary are the tidal ebb and flow and the fresh-water discharge, which are constantly at work; and they regulate the size of the channels in an estuary, and for the most

part their direction, as well as the limits of accretion. These are the forces which can be reproduced in miniature in a model, as proved by the close concordance in the channels obtained by experiment with the actual conditions of the Mersey, and with a previous state of the Seine estuary; and this similarity of results would not have occurred if the other influences noticed above were at all equally potent.

Training walls mainly modify the direction and action of the tidal ebb and flow and fresh-water discharge; and, therefore, it is reasonable to suppose that the results in a model, due to these alterations, would correspond to their actual effects in an estuary, provided the important element of accretion could be also reproduced. This was satisfactorily accomplished in the second stage of the investigation, proving that the miniature influences produced in the model corresponded, in this case also, with the forces acting in the estuary. Accretion is promoted by training walls in an estuary where matter is carried in suspension; but the action of waves in modifying the channels is stopped by the intervention of training walls. Accordingly, the further the training walls are extended, and the more an estuary is protected by works such as those indicated in Plate 2, the more is the modifying influence of waves eliminated, and therefore the more are experiments in a model likely to correspond with the conditions of estuaries under similar conditions.

Other considerations also afford grounds for supposing that the effects observed with training walls in a model fairly correspond with the results which such works would produce in an estuary. The charts of the experiments show that definite results followed from certain lines inserted in the model, and that modifications in these lines were followed by modifications in the results. (Compare Plate 2, figs. 1, 2, and 3, and Plate 3, fig. 2, with Plate 4, fig. 1.) Moreover, the results produced with the model agree very closely with the results which, in the two earliest schemes experimented upon, it was stated, before the experiments were begun, would follow, if the works indicated by lines in the charts were actually carried out in the Seine estuary.*

* Compare the observations relating to Scheme A and Plate 2, fig. 1, with the following extract from 'Instit. Civ. Engin. Proc.,' vol. 84, p. 356:—"The narrowing of the mouth of the estuary of the Seine would at first promote scour, and increase the depth in that part of the channel, and for a little distance above and below. This contraction, however, would impede the influx of the flood tide, and cause changes in the velocity of the current through the narrow neck, and in the wide estuary above, promoting the deposit of silt brought in by the tide. This accretion would be greatly aided by the prolongation of the training walls to Honfleur, so that eventually the greater portion of the estuary comprised between Tancarville, Hoc Point, and Honfleur would be raised to high-water level. This large reduction in tidal capacity would reduce the tidal current through the narrowed entrance, and consequently diminish again the depth in the channel. Moreover, this reduction of

It would be impossible to determine by experiment the time any changes in an estuary would occupy. The figures, in fact, giving the number of tides during which each experiment was worked, are not even intended as an indication of the rate of change in the model, and much less as any measure of the period required for such changes in an estuary, but merely as a record of the comparative duration of each experiment. It was observed, however, that the changes were most rapid where the modifications effected by the lines of walls inserted in the model were greatest (Plate 2), and slowest where the lines in the model produced the least alterations. (Plate 3, fig. 1, Plate 4, fig. 3.)

Principles for Training Tidal Rivers deduced from Experiments.

The foregoing investigations, viewed merely as experiments, without any reference to their bearing on the Seine, may serve for indicating some general principles applicable in training tidal rivers through wide estuaries. Direct experiment for each estuary is undoubtedly preferable to abstract reasoning, where such experiment is possible, as it reproduces the special conditions of the estuary to be investigated. Nevertheless, general principles may be of value in guiding the choice of designs to be investigated, so as to avoid waste of time in testing unfavourable schemes, and also in cases where the conditions of an estuary are not sufficiently known to afford a correct basis for experiment.

The experiments may be divided into three classes, namely:—

(1.) Outlet of estuary considerably restricted, and channel trained inside towards outlet. (Plate 2.)

(2.) Channel trained in sinuous line, expanding towards outlet, but kept somewhat narrow at changes of curvature. (Plate 3, figs. 2 and 3, and Plate 4, figs. 1 and 2.)

(3.) Channel trained in as direct a course as practicable, and expanding regularly to outlet. (Plate 3, fig. 1, and Plate 4, fig. 3.)

The experiments of the first class exhibited a deep outlet, and a fairly continuous channel inside, where the training works were prolonged to the outlet. The channel, however, was irregular in depth

tidal flow in and out of the lower estuary would favour the natural heaping-up action of the sea on the sands outside; so that eventually, not only would the initial deepening of the narrowed outlet be lost, but the good depths in the bay outside the estuary would be imperilled."

Compare also Plate 3, fig. 1, with the following extract from 'Instit. Civ. Engin. Proc.,' vol. 84, p. 250:—"The continuously concave southern training wall, whilst very favourable to Honfleur, will unduly keep the ebb current to that side, and therefore away from Havre. Also, the extension of the wall along the Ratier Bank will act like a groyne, and, arresting the silt-bearing southern current, will connect Trouville Bank with the shore, and lead to a large accumulation of deposit in front of Trouville . . . . and also the low walls proposed will not prevent accretion."

Fig 1

SCHEME A

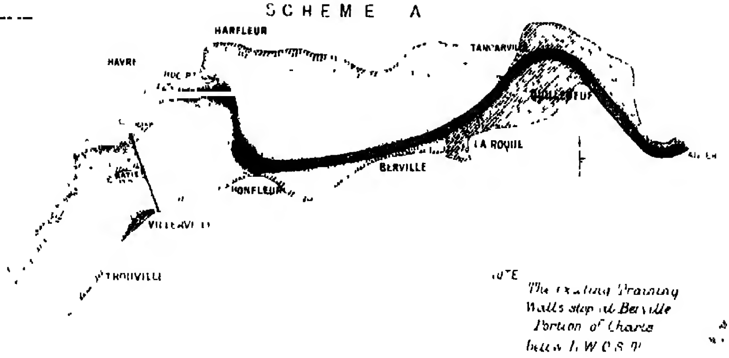


Fig 2

SCHEME B1

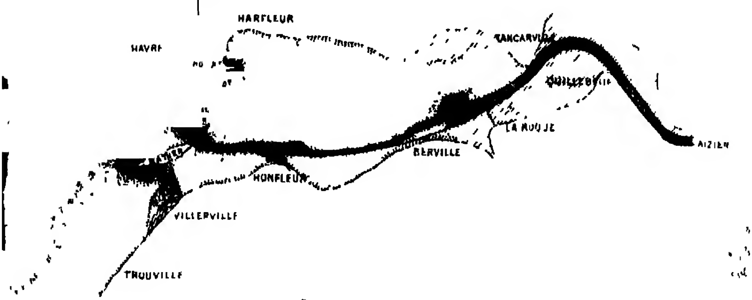


Fig 3

SCHEME B2

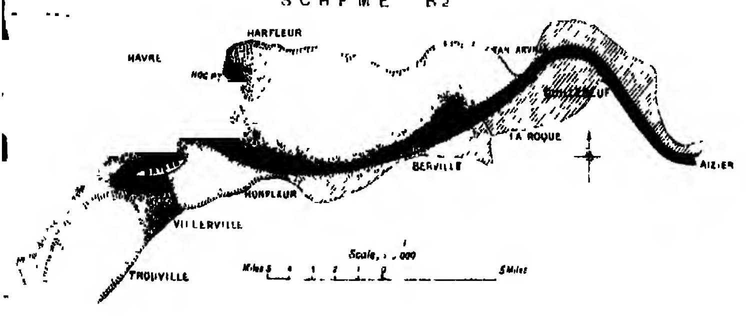
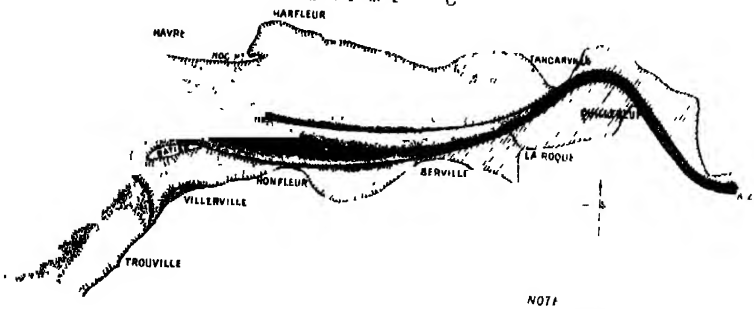


Fig 1
SCHEME C



NOTE

Fig 2
SCHEME D

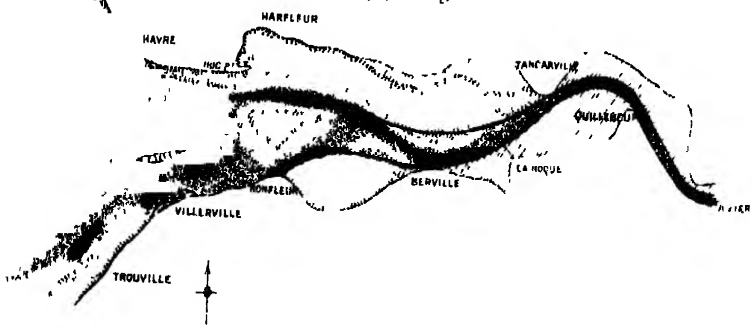


Fig 3
SCHEME D bis

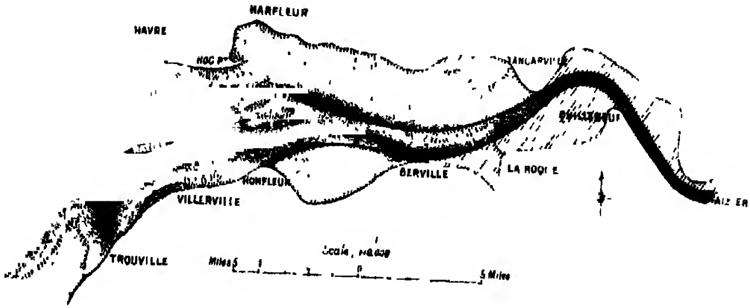


Fig 1
SCHEME E

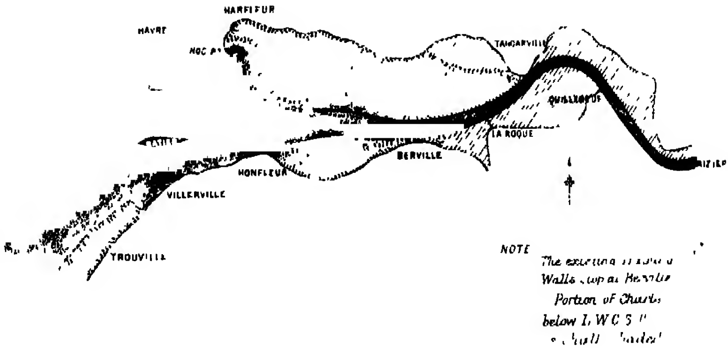


Fig 2
SCHEME E bis

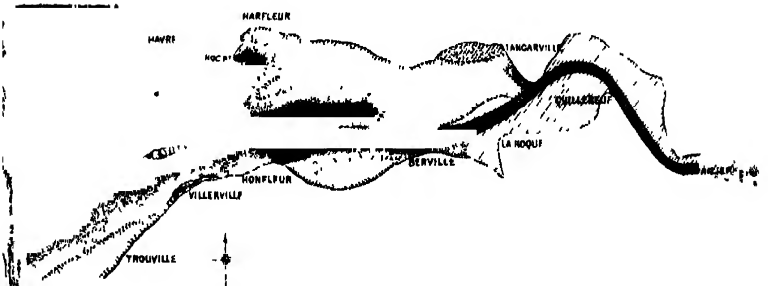
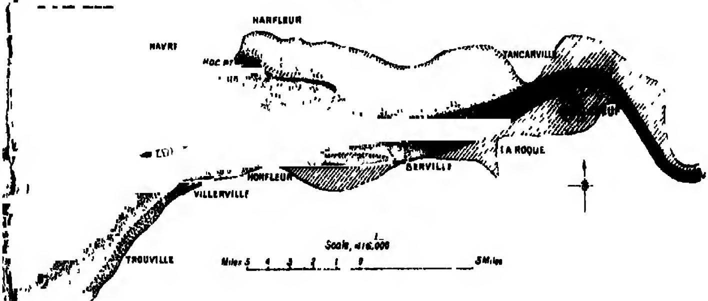


Fig 3
SCHEME F



West, Newman & C° lith.

near the outlet; and a bar appeared in front of the outlet outside. The breakwater also, extending across part of the outlet, favoured deposits both inside and outside the estuary, by producing slack water in the sheltered recesses.

The second class of trained channel was designed to profit by the scour at the concave face of bends, so clearly exhibited at the first bend of all the charts, and to continue the depth thus obtained by restricting the width between the bends, on the principle adopted for winding non-tidal rivers. Experiment, however, did not bear out the advantages anticipated from this system, probably owing to the variable drection of the flood tide at different heights of tide, its being checked in its progress by the winding course, and not acting in unison with the ebb from the difference in its direction and the width of the trained channel near the outlet. The main stream in a non-tidal winding river always follows a tolerably definite course; whereas the flood tide tends gradually, as it rises, to assume as direct a course as possible. The difference, therefore, in the conditions of a non-tidal and tidal river, in this respect, is considerable.

The third class of trained channel afforded a wide, tolerably uniform channel in the experiments; the flood tide was less impeded in its progress than with the other forms of training walls, and appeared to act more in concert with the ebb.

The experiments, accordingly, indicate that the only satisfactory principle for training rivers, through wide estuaries with silt-bearing currents, is to give the trained channel a gradually expanding form, with as direct a course as possible to the outlet. The rate of increase of width between the training walls must be determined by the special conditions of the estuary. If the outlet is very wide, and the gradual expansion in width cannot be commenced a considerable distance up an estuary, some restriction in width at the outlet may be expedient to avoid a too rapid expansion. It is evident that the widening out adopted in the last experiment (Plate 4, fig. 3) was carried to its utmost limits, from the continuance of sandbanks inside the trained channel, and that, regarding merely the improvement of the channel, it might have been preferable to restrict its width at the outlet as effected in Scheme C (Plate 3, fig. 1). At the same time, it must not be inferred, from the existence of these sandbanks, that the distance apart of the training walls was much too great in the last experiment; for the width apart of the training walls necessitated the inclusion of a greater extent of sandbanks within the trained channel at the outset, and also rendered the rate of improvement in the channel more gradual, so that the improvement in the channel both in direction and depth was still progressing at the close of the experiment, and the sandbanks in the channel were in process of removal, and not being formed. The choice in such cases, where the

widening out cannot be commenced far up, appears to lie between the utmost improvement of the channel at the expense of accretion on the foreshores outside, and the maintenance of the depths over the foreshores beyond the outlet accompanied with a somewhat less good channel in the estuary. In some cases, deposit on the foreshores at the side beyond the outlet might be of no importance, and then the river channel should be primarily considered; but if, on the contrary, accretion on the foreshores outside is undesirable, the outlet must be maintained by a greater widening out of the training walls. The actual direction of the training walls must be determined, in each case, by the general direction of the channel above, the situation of ports on the estuary, the position of the outlet, and the set of the flood tide at the entrance.

Concluding Remarks.—In terminating this record of my investigations, and the general principles for training works which they seem to indicate, I desire to acknowledge the care with which my assistant, Mr. E. Blundell, has carried out the tedious task of working the tides in the model, and prepared the charts of the experimental results from which the illustrations accompanying this paper have been drawn out. Eddies at sharp edges, due to distortion of scale, appear to have excessive scouring effect in a model; whilst the action of the more regular currents exhibits a deficiency in scouring power, as previously noted. Though the actual depths of the channels, however, are too small for the distorted vertical scale, reliance, I think, may be placed on the general forms and relative depths of the channels obtained in a model. It is possible that the inadequate depth might be remedied by the employment of a finer or lighter material for forming the bed of the model, or by using a liquid of greater density than water; but sand and water have the unquestionable advantage of being the substances which actually effect the changes in estuaries.

"On the Cranial Nerves of Elasmobranch Fishes. Preliminary Communication." By J. C. EWART, M.D., Regius Professor of Natural History, University of Edinburgh. Communicated by Professor BURDON SANDERSON, F.R.S. Received February 22,—Read March 7, 1889.

Although the cranial nerves of *Hexanchus*, *Echinorhinus*, and *Scyllium* have been fully described, and the segmental value of the nerves of Elasmobranch fishes repeatedly considered, the nervous system of *Læmargus* has hitherto escaped notice. This is probably to be accounted for by anatomists taking for granted that *Læmargus* agreed in the arrangement of its nerves with *Echinorhinus* and other *Spinacidæ*.

I have not yet had an opportunity of comparing *Læmargus* with either *Hexanchus* or *Echinorhinus*, but I have satisfied myself that the accounts given of the cranial nerves of these forms are not applicable in several important respects to the cranial nerves of *Læmargus*, nor yet to the nerves of the common skate (*Raia batis*). Further, I find that when, having mastered the arrangement of the cranial nerves of *Læmargus* and *Raia*, one turns to *Petromyzon*, *Scyllium*, *Galeus*, and other familiar forms, it is impossible to accept many of the statements hitherto made as to the nature, distribution, and segmental value of the cranial nerves of vertebrates.

In this preliminary communication I propose to describe shortly the cranial nerves of *Læmargus* and *Raia*, reserving for a future paper a comparison between the nerves of *Læmargus* and other Elasmobranchs, and the consideration of the segmental value and the more important modifications of the cranial nerves in the chief subdivisions of the vertebrate group.

I. *The Cranial Nerves of* Læmargus.

As the olfactory and optic nerves closely resemble those of *Hexanchus*, it is unnecessary to refer to them in this preliminary note, and instead of beginning, as is usually done, with the oculo-motor, I shall first describe the ophthalmicus profundus.

1. *The Ophthalmicus Profundus.*—This nerve has usually been said to belong either to the oculo-motor or to the trigeminal. It presents a root, more or less distinct, a root ganglion, and a trunk which gives off a number of well-marked branches. Although the segmental value of the ophthalmicus profundus need not now be discussed, it may be mentioned that since van Wijhe demonstrated that it possessed a ganglion, its right to rank as a separate cranial nerve has been deemed worthy of consideration. Although Marshall and Spencer concluded that there was nothing in support of the view that the root of this nerve belonged to the trigeminal, and believed that its trunk was a branch of the oculo-motor, Gegenbaur has recently stated that he considers the ophthalmicus profundus with its ganglion as part of the trigeminal. Very different views have been held as to the ganglion of the ophthalmicus profundus. By Marshall and Spencer the ganglion was said to belong to the oculo-motor, and was identified as the ciliary ganglion. Beard, on the other hand, considers the ganglion of the ophthalmicus profundus as homologous with the Gasserian ganglion, while he thinks the ciliary ganglion probably corresponds to a sympathetic ganglion. Believing, with van Wijhe, in the possible existence of two ganglia, one on the ophthalmicus profundus and one (the ciliary) in connexion with the oculo-motor, Beard has given to the ganglion of the ophthalmicus

profundus the name of *meso-cephalic*. He further states that there can be no doubt that the ciliary (as distinguished from the meso-cephalic) ganglion of lower vertebrates is exactly homologous with the ciliary ganglion of mammals.

The ophthalmicus profundus nerve (1, fig. 1) in *Læmargus* arises by several rootlets (2—5) from the side of the medulla immediately in front of the main root of the trigeminal, and runs outwards in contact with the anterior surface of the trigeminal to enter the large foramen which serves for the passage of the trigeminal, and the anterior portions of what may best be known as the facial complex. As the ophthalmicus approaches the foramen it partly blends with the trigeminal, and while in the foramen it communicates with this nerve by

several small branches. On escaping from the cranial wall the ophthalmicus profundus separates from the trigeminal and presents a slight swelling, the meso-cephalic ganglion (Beard), or ciliary ganglion (Gegenbaur and others). This ganglion lies dorsal to, but only very slightly in front of, the large Gasserian ganglion of the trigeminal (3, fig. 1). From the ganglion the trunk extends forwards over the external rectus muscle to pass under the rectus superior towards the eyeball, from which it bends inwards between the superior oblique and internal rectus muscles, to reach the snout by penetrating the pre-orbital process of the cranium.

The more important branches of the ophthalmicus profundus are (1), a small branch which passes outwards above the superior rectus muscle; (2), two or three ciliary branches (*ci.*, fig. 1), which run forwards under cover of the rectus superior to enter the eyeball—to these ciliary branches delicate filaments pass from the deep branch of the oculo-motor; (3), small branches which pass outwards in front of the eyeball; (4), branches to the skin, and subcutaneous tissue of the snout and to the rostral cartilage. I have been unable to trace any branches from the ophthalmicus profundus to either the mucous canals or the ampullæ of the sensory tubes: long and slender branches, however, seem to be distributed to the tubes which extend from the ampullæ to open through the skin.

2. *The Oculo-motor Nerve.*—Although this nerve does not necessarily stand in the same relation to the ophthalmicus profundus as does the ventral root to the dorsal root of a spinal nerve, it will be convenient to deal with it before considering the trigeminal. The oculo-motor has been ranked very differently by different observers. Marshall and Spencer considered it of segmental value, and Gaskell has recently stated that it retains in its root vestiges of a ganglion. Van Wijhe looks upon the oculo-motor as forming the ventral (motor) root of the ophthalmicus profundus, whilst Gegenbaur neither admits that it has the rank of a segmental nerve nor feels satisfied that it represents the ventral root of the ophthalmicus profundus.

The oculo-motor (2, fig. 1) in *Læmargus* arises by a number of delicate rootlets (5—7) from the under-surface of the mid-brain, on a level with the posterior end of the optic lobes and in line with the abducens and spinal nerves. Passing outwards it escapes from the cranial cavity by a special foramen, and bends round the orbital process of the palato-pterygoid arch to reach the rectus superior, where it divides into a superficial and a deep branch. The superficial supplies the superior and internal recti muscles, the deep branch passing under the rectus superior sends filaments to the inferior rectus and inferior oblique muscles, and, as it runs over the pedicle, it sends one or two exceedingly delicate twigs to the ciliary branches of the ophthalmicus profundus. I have been unable to find any

ganglionic cells that might represent a root ganglion in any part of the oculo-motor nerve or any representative of a ciliary ganglion, in addition to the ganglion of the ophthalmicus profundus, or even any communication between the oculo-motor nerve and the ganglion of the ophthalmicus profundus, which has apparently been often described as the ganglion of the oculo-motor nerve, *i.e.*, as the ciliary ganglion.

3. *The Trigeminal Nerve.*—Hitherto anatomists have, with few exceptions, described the trigeminal nerve as arising in Elasmobranchs by several roots, but there has seldom been complete agreement as to either the number or position of the roots, and hence great confusion has arisen. Marshall and Spencer did much to remove this confusion by showing that the so-called dorsal root of the trigeminal undoubtedly belonged to the facial. They described the trigeminal as arising by a small anterior non-ganglionic root and a large posterior ganglionic root. Their small anterior root evidently corresponds to the root of the ophthalmicus profundus, the ganglion of which they transferred to the oculo-motor.

In *Læmargus* the origin of the trigeminal (3, fig. 1) is easily made out. When the rootlets of the ophthalmicus profundus are removed, the trigeminal is found to spring from the side of the medulla by a single large root (the posterior root of Marshall and Spencer), which lies in a line with the ventral roots of the facial complex. The root of the trigeminal passes forwards and, blending with the ophthalmicus profundus, enters the foramen under cover of two of the subdivisions of the facial complex, viz., the ophthalmicus superficialis and buccal. As it passes through the foramen it presents a distinct swelling—the Gasserian ganglion. The trunk of the nerve at once divides into two large branches—the maxillary and mandibular. A third but slender branch (the superficial ophthalmic branch of the trigeminal) springs either from the trunk or from the mandibular. Two very slender nerves, which leave the root as it crosses the cranial cavity, pass upwards through the walls of the cranium towards the skin in front of the ear capsule.

The branches of the trigeminal are: (1) the superficial ophthalmic which runs first along the inner and then obliquely over the upper surface of the ophthalmicus superficialis of the facial complex, to pass through a special canal in the pre-orbital cartilage and send branches to the subcutaneous tissue of the snout, especially in front of the preorbital process. (2) The maxillary branch. This nerve runs forwards and outwards under the eye muscles, dividing on the way into branches, which reach the under surface of the snout and terminate in the vicinity of the anterior labial and palato-pterygoid cartilages. The trunk and its various branches are intimately related to the buccal subdivision of the facial complex. (3) The

mandibular branch. This large nerve first gives off a number of small twigs which pass under the buccal division of the facial to assist in supplying the muscles in front of the spiracle. It then divides into branches which pass forwards and outwards supplying the mandibular and other muscles, and finally sends branches to the skin in the vicinity of the mandibular arch and the posterior labial cartilage. Some fibres from both the maxillary and mandibular nerves penetrate between the sensory tubes, and lie in close contact with the mucous canals, but in no case have I found them terminating in the ampullæ or penetrating the mucous canals to end in the sensory tissue lodged in their cavities.

The Facial Complex.—In describing the cranial nerves of *Hexanchus*, Gegenbaur considered the trigeminal and facial nerves as forming a single group, and he included amongst the roots of the trigeminal the roots of two nerves (ophthalmicus superficialis and buccal) now all but universally acknowledged as belonging to the facial.

While in the higher vertebrate the trigeminal nerve is of far more importance than the facial, in the lower fishes it is otherwise; for while the trigeminal proper consists of but a single root the so-called facial is made up of three large roots, one of which seems to be double. Hence, instead of grouping the trigeminal and facial nerves together, it will be more convenient to consider the facial nerves by themselves and speak of them as the facial complex. This complex includes four separate nerves, viz., (1) the ophthalmicus superficialis, (2) the buccal, (3) the palatine, and (4) the hyomandibular. In the meantime it is only necessary to mention that the enormous development of the so-called facial is owing to the presence of a complex system of lateral sense organs—sensory tubes and mucous canals.

4. *The Ophthalmicus Superficialis.*—This nerve (4, fig. 1) arises by a large root from the so-called trigeminal nucleus which occupies the most dorsal portion of the medulla. The root, in a large fish, lies on a higher level (by about 4 mm.) than the other roots of the facial complex, and it is also the most posterior root, *i.e.*, the furthest from the snout. Arising far apart from the other divisions of the facial it runs forwards and then bends downwards to reach the buccal nerve, with which it freely communicates as it passes through the cranial walls at a higher level than the trigeminal and ophthalmicus profundus. Immediately beyond the walls of the cranium it presents a ganglionic swelling, which consists of large bipolar cells, similar to those of the Gasserian ganglion. The main trunk of the nerve then arches round the conical orbital process of the palato-pterygoid arch, and extends forwards above the eye muscles to send branches to the sensory tubes and mucous canals of the snout.

In *Læmargus* the ophthalmicus superficialis of the facial supplies (1) the ampullæ of the sensory tubes on the dorsal aspect of the snout,

and (2) the cranial, rostral, subrostral, and nasal mucous canals.* These canals are described by Garman, one of the latest writers on the subject, as being supplied by the trigeminal. It may be mentioned that the minute branches for the cranial canal spring from the trunk of the nerve as it passes through the orbit and reach the surface by piercing the cartilage of the roof of the orbit at short and nearly regular intervals. A remarkable bundle of fibres runs obliquely across the upper border of the ophthalmicus superficialis at its origin, and reaching its anterior surface turns abruptly downwards to lie first in front of and afterwards under the buccal nerve. These fibres then form a plexus from which numerous twigs proceed to the conjoined roots of the hyomandibular and palatine nerves; they probably eventually reach and end in ampullæ and mucous canals.

5. *The Buccal Nerve.*—This nerve (5, fig. 1) springs by a large root from the side of the medulla, behind and on a slightly higher level than the root of the trigeminal. As the root passes outwards, it lies in the groove formed by the roots of the trigeminal and the posterior portion of the facial complex. After communicating freely with the ophthalmicus superficialis, it escapes with it through the cranial walls. Leaving the ophthalmicus superficialis, it comes into close contact with the outer surface of the Gasserian ganglion, and then lies between the maxillary and mandibular branches of the trigeminal. As the buccal nerve leaves the Gasserian ganglion, it presents a distinct swelling which is crowded with large bipolar cells. This may be called the buccal ganglion. The buccal nerve beyond the ganglion comes into intimate relation with the maxillary nerve, and as it runs forward under the contents of the orbit, it breaks up into branches which eventually reach the ampullæ and mucous canals of the snout not supplied by the ophthalmicus superficialis. The buccal nerve also sends branches to the anterior portion of the occipital mucous canal, and to the posterior part of the cranial mucous canal, and it sends a branch backwards which disappears under the hyomandibular cartilage. Further, by means of branches which run outwards, behind or under the contents of the orbit, the buccal nerve supplies the orbital and suborbital canals, apparently without any assistance from the maxillary and mandibular branches of the trigeminal.

The Palatine and Hyomandibular Nerves.—These nerves arise by a large root which lies between the trigeminal and auditory nerves, and partly under cover of the buccal nerve. This root is augmented by fibres from the plexus which, as mentioned above, is formed in connexion with the bundle of fibres that arches downwards from the ophthalmicus superficialis. Having received these additional fibres,

* The names used for the mucous canals are those of Agassiz as modified by Garman.

the common root arches backwards, and enters a large foramen along with the auditory nerve. Leaving the auditory, it runs forwards through a canal in front of the auditory capsule. Having proceeded some distance (about 5 cm. in a large fish), it divides into two branches, a large branch (the hyomandibular) that proceeds outwards behind the spiracle, and a smaller branch (the palatine), which bends downwards towards the roof of the mouth. When the common trunk of these nerves is carefully studied, it is found to consist of two separate bundles, a small bundle which seems to be continuous with the palatine nerve, and a larger bundle which is continuous with the hyomandibular nerve. At the point of bifurcation there is a large collection of ganglionic cells, some of which lie in the palatine nerve and may be known as the palatine ganglion. Further, the two nerves are connected in front of the apparently compound ganglion by a number of fibres which have a somewhat plexiform arrangement.

6. *The Palatine Nerve.*—This nerve (6, fig. 1) at once gives off a number of branches (prespiracular) which are distributed to the tissues in front of the spiracle. The main trunk sends numerous branches to the roof of the mouth. Continuous with what may be known as the root of the palatine nerve, a distinct bundle of fibres runs outwards under the hyomandibular nerve (from which it receives one or more small branches), and passing over the hyomandibular cartilage, runs forwards to end in the fold of mucous membrane lying between the hyoidean and mandibular cartilages. I look upon this long slender nerve as corresponding to the chorda tympani of higher vertebrates.

7. *The Hyomandibular Nerve.*—This nerve (7, fig. 1) which increases immensely in size, beyond the ganglion, is chiefly concerned in supplying the large group of ampullæ that lies external to the spiracle, but it also supplies the mucous canals not already referred to, with the exception of the aural mucous canal and the canal of the lateral line. It further sends a branch backwards to muscles lying over and within the hyomandibular cartilage and the branchial apparatus.

In describing the facial complex, I have referred to a special ganglion on the ophthalmicus superficialis, to another on the buccal, and to a compound ganglion in connexion with the hyomandibular and palatine nerves. Gegenbaur considers the palatine nerve of Elasmobranchs as homologous with the great petrosal nerve of mammals. If this comparison holds, which I have every reason to believe it will, the interesting question arises—Is there any relation between the palatine ganglion of the Elasmobranch and the spheno-palatine ganglion of the mammal? And this leads to the further question—Are the ganglia of the ophthalmicus superficialis, buccal, and hyomandibular nerves related to the geniculate, otic, and submaxillary ganglia

of the higher vertebrates? These and other questions I shall hope to deal with in a future paper.

8. *The Trochlearis Nerve.*—This nerve (8, fig. 1) arises from the side of the brain immediately behind the optic lobe. It passes forward and upwards to pierce the cranium a considerable distance in front of the oculo-motor, it then dips downwards and outwards under the ophthalmicus superficialis to supply the superior oblique muscle. I have been unable to find any sensory branch passing from this nerve in *Lœmargus*, and in no part of its length does it contain ganglionic cells.

9. *The Abducens.*—This nerve (9, fig. 1) has a striking resemblance to the anterior spinal nerves. It arises by three or four extremely slender rootlets which are in a line with the rootlets of the oculo-motor in front and the spinal nerves (ventral roots) behind. The rootlets unite to form a trunk which at first lies midway between the auditory and glossopharyngeal nerves. The trunk proceeds forward and perforates the cranial wall to reach and supply the external rectus muscle. The abducens nerve, like the oculo-motor and trochlearis, is devoid of ganglionic cells. It cannot be said to be specially related to the facial complex—to form as has been suggested its motor root.

10. *The Auditory Nerve.*—The auditory nerve (10, fig. 1) lies immediately behind and slightly ventral to the common root of the ventral portion of the facial complex. It runs outwards behind these nerves and enters the same cranial canal and at once divides into branches for the auditory apparatus. Although there is no distinct swelling, the root, some distance from its origin, is crowded with ganglionic cells.

11. *The Glossopharyngeal Nerve.*—This nerve (11, fig. 1) has been long considered one of the most primitive and typical of the cranial nerves. It arises from the side of the medulla in front of and in a line with the rootlets of the middle portion of the vagus, but under cover of the anterior portion of the vagus. The number of rootlets varies, but there is usually one large rootlet and two or three slender ones, and it receives a twig from one of the rootlets of the anterior portion of the vagus. The rootlets together form a small rounded nerve, which passes backwards and outwards through a special canal under the auditory capsule to reach and give two large branches (pre- and post-branchial) to the walls of the first true branchial cleft and a small branch (pharyngeal) to the pharynx. When midway through the walls of the cranium it presents a distinct swelling which is crowded with ganglionic cells. Immediately beyond the ganglion a small dorsal branch takes its origin, which passes upwards through the cranium to reach the skin over the auditory region. Apparently this dorsal branch does not assist in supplying either mucous canals or sensory tubes.

The Vagus Complex.—The vagus has been long held to represent a large number of nerves which, in most vertebrates, gradually coalesced as the branchial region became reduced in size or otherwise altered. Balfour states that the vagus arises in Elasmobranchs by four ganglionic roots, while more recently Beard and van Wijhe agree in describing the vagus as first appearing in the form of an unsegmented band which later blends with an epiblastic sensory thickening above the four posterior branchial clefts. The nerve for the second true branchial cleft is said, at an early period, to separate from this mass and develop a ganglion. Later the three posterior branchial nerves appear, but for these it is said there is usually only a single ganglion which, however, ventrally "shows a division into three portions." While the anterior portion of the vagus is described as supplying the second branchial cleft, the nerve to the lateral line is described as arising as a secondary formation from the epiblastic sensory thickening above mentioned. The lateral line nerve is usually described as springing from the common trunk, but Balfour, impressed with the importance of this nerve, says it "may very probably be a dorsal sensory branch of the vagus." That this surmise is practically correct will appear from what follows.

In *Læmargus* the vagus complex (12, fig. 1) arises by numerous rootlets disposed in three separate groups, an anterior group including two or three rootlets, a middle consisting of over twenty, and a posterior group of five or six rootlets. Hitherto the anterior portion of the vagus has been usually spoken of as Vagus I, or the nerve of the second branchial (first vagus) cleft. In *Læmargus* the anterior division of the vagus appears to be almost entirely concerned in supplying the mucous canal of the lateral line, and hence it may be known as the lateralis nerve, or nerve of the lateral line. Its right to be considered as a special nerve becomes all the more evident when it is mentioned that in some, if not all cases, it presents a ganglionic swelling. The lateralis nerve (*l*, fig. 1) seems in many respects comparable to the ophthalmicus superficialis of the facial complex, and like this latter nerve it springs from the side of the medulla on a higher level than the other divisions of the group to which it belongs. In several cases I have found it arising by one large root and a small accessory rootlet dorsal to and slightly in front of the roots of the glossopharyngeal. Having sent a twig from its small rootlet to the glossopharyngeal, it extends backwards to enter and traverse with the rest of the vagus the long cranial canal that runs backwards and outwards behind the auditory capsule. Soon after entering the canal it seems to blend with the rest of the vagus, but with care the whole or at least most of the fibres springing from above the glossopharyngeal can be shown to be directly continuous with the nerve of the lateral line. Soon after entering the canal it gives off a slender branch which,

leaving the lateralis, arches upwards to supply the aural mucous canal and the anterior portion of the canal of the lateral line. Before escaping from the cranium the lateralis gives off another slender branch which is distributed to the succeeding portion of the lateral line. The rest of the lateral line is supplied by numerous slender fibres which spring from the lateralis as it passes backwards towards the tail.

In addition to the lateralis there are five other nerves in *Læmargus*, belonging to the vagus complex, viz., an intestinal and four branchial nerves. The first branchial nerve (the Vagus I of most authors), which is made up of the rootlets which lie immediately behind the root of the glossopharyngeal nerve, lies at first in close contact with the lateralis. This nerve (2*b*, fig. 1) presents a distinct ganglionic swelling as it passes through the vagus canal. Before escaping from the canal it breaks up into the three characteristic branches—post- and pre-branchial and pharyngeal. The three posterior branchials (3—5 *b*, fig. 1) and the intestinal (*i*, fig. 1) are derived from the common trunk. This trunk contains numerous ganglionic cells. In a large fish the compound ganglion (*g*, fig. 1) may reach a length of six or seven inches. Each of the branchials gives off the three usual branches, while the intestinal passes backwards towards the intestine and other structures. From the common trunk three or four slender filaments which extend outwards at a deeper level than the branches of the lateralis may represent dorsal branches of the posterior branchial nerves. It may be added that the vagus complex has no ventral roots; the so-called ventral roots of the vagus represent spinal nerves which have probably lost their posterior roots. In their distribution these nerves (1—2 *sp.*, fig. 1) agree with spinal rather than with cranial nerves; two of them penetrate the occipital region of the skull on their way to the surface.

II. *The Cranial Nerves of* Raia batis.

The cranial nerves of the skate, with the exception of those belonging to the vagus complex, closely resemble the corresponding nerves of *Læmargus*, hence, with the exception of the vagus, little more is necessary in the meantime than a short reference to their respective ganglia.

1. *The Ophthalmicus profundus.*—The root of this nerve (1, fig. 2), in *Raia* is more intimately connected with the root of the trigeminal than in *Læmargus*. The position and relations of the ganglion are of special interest. In *Læmargus* the ganglion of the ophthalmicus profundus was situated some distance behind, and it had no connexion with, the oculo-motor nerve. In *Raia* the ganglion of the ophthalmicus profundus lies some distance in front of the Gasserian ganglion, partly under cover of the rectus superior muscle and over the deep branch

of the oculo-motor. Further, the ciliary nerves, instead of springing from the trunk of the nerve some distance in front of the ganglion, as in *Læmargus*, usually spring from the under surface or outer margin of the ganglion, and hence the branches (ciliary) of the oculo-motor nerve, in passing to join the ciliary branches of the profundus, have to pass under the ganglion of the ophthalmicus profundus; the ganglion of the ophthalmicus profundus thus seems to be in a sense connected with the oculo-motor nerve, which doubtless explains why so many observers have described the ganglion of the ophthalmicus profundus as belonging to the oculo-motor. Were the root and trunk of the ophthalmicus reduced to slender filaments, the conditions found in some of the higher vertebrates would be arrived at, and the ganglion of the ophthalmicus profundus would appear to belong to the oculo-motor rather than to a branch of the trigeminal or an entirely separate nerve.

The oculo-motor, pathetic (2, 3, fig. 2), and abducens resemble the

corresponding nerves in *Lœmargus*, and, as in *Lœmargus*, they never present ganglia in any part of their course.

There is the usual ganglion on the trigeminal nerve, and this nerve (3, fig. 2), as in *Lœmargus*, divides into maxillary (3′, fig. 2) and mandibular (3″, fig. 2) branches, and sends a superficial ophthalmic branch to the snout along with the ophthalmicus superficialis of the facial. The facial complex, again, consists of four nerves, viz.:—(1) the ophthalmicus superficialis (4, fig. 2), with a large ganglion, which lies immediately above and in front of the Gasserian ganglion; (2) the buccal (5, fig. 2), with a ganglion lying over the origin of the mandibular branch of the trigeminal; (3) the palatine (6, fig. 2), (with an indistinct root containing ganglionic cells) which gives off palatine and prespiracular branches, and a branch which extends outwards to unite with fibres from the hyomandibular and bend round the hyomandibular cartilage, and eventually reach the floor of the mouth behind the mandible, thus resembling the chorda tympani; and (4) the hyomandibular nerve (7, fig. 2), which is chiefly distributed to mucous canals and the ampullæ of sensory tubes. To the hyomandibular a large bundle of fibres is contributed, as in *Lœmargus*, from the upper border of the ophthalmicus superficialis.

The auditory nerve (10, fig. 2) lies in contact with the hyomandibular, and has numerous ganglionic cells in its root.

The glossopharyngeal nerve (11, fig. 2) runs forwards from under the root of the lateralis nerve, and, bending backwards, passes outwards through a canal which opens into the floor of the cavity of the auditory capsule. Passing through the cavity of the capsule, the nerve next penetrates its outer wall, and at once expands to form a large oval ganglion, from which the usual branches take their origin. A dorsal branch, which reaches the surface of the head, does not seem to supply any portion of the occipital or aural mucous canals.

The vagus complex (12, fig. 2) in some respects seems to be more primitive in the skate than in any other Elasmobranch. It may be said to consist of six nerves, all of which can be readily distinguished, and each nerve presents a distinct ganglion. These nerves are (1) the lateralis (*l*, fig. 2), which springs by a special root above and in front of the glossopharyngeal nerve. The ganglion of the lateralis varies in position, being sometimes situated nearly two inches beyond the point at which the nerve issues from the cranium, in other cases only half an inch from its point of exit. A slender branch arising in the vagus canal arches upwards to supply the anterior portion of the canal of the lateral line, the aural, and part of the occipital mucous canals. The main trunk of the lateralis is distributed to the rest of the canal of the lateral line and to the posterior pleural canal. (2.) The four branchial nerves. The first three branchial nerves (2—4 *b*, fig. 2) acquire an independent existence almost as soon as the

vagus escapes from the cranium, while the fourth (5 *b*, fig. 2) is united with the intestinal nerve (*i*, fig. 2) until the level of the fifth branchial cleft is reached. In each of the three anterior branchial nerves the ganglion is situated within a short distance of the point of bifurcation into the post- and pre-branchial branches. The ganglion of the fourth nerve lies in contact with the common trunk from which it springs. (3.) The intestinal nerve (*i.*, fig. 2) passes backwards, and has its ganglion immediately beyond the point where it separates from the last branchial. Sometimes one (1 *sp.*, fig. 2), or two ventral roots of spinal nerves arise under cover of the roots of the vagus and escape through the occipital region of the cranium, but as in *Læmargus* they never unite with any of the fibres of the vagus, and there is no reason for considering them as ventral roots of the vagus complex.

I am indebted to Mr. Sim, naturalist, Aberdeen, for the specimens of *Læmargus* required for the investigation.

The following list includes the more important papers and works referred to :—

Balfour. 'A Monograph of the Development of Elasmobranch Fishes.'
'Comparative Embryology.'

Beard. "Sense Organs of Ichthyopsida," 'Quart. Journ. of Microsc. Science,' 1885.

"The Ciliary and Motor-oculi Ganglion and the Ganglion of the Ophthalmicus profundus in Sharks," 'Anat. Anzeiger,' 1887, Nr. 18—19.

"The Development of the Peripheral Nervous System of Vertebrates," 'Quart. Journ. Microsc. Science,' October, 1888.

Froriep. 'Anat. Anzeiger,' vol. 2, 1887.

Garman. "On the Lateral Canal System of Selachia and Holocephala," 'Bulletin Mus. Comp. Zool., Cambridge, Mass.,' vol. 17, No. 2.

Gaskell. "On the Relation between the Structure, Function, and Distribution of the Cranial Nerves," 'Roy. Soc. Proc.,' vol. 43.

Gegenbaur. "Die Kopfnerven von Hexanchus," 'Jenaische Zeitschrift,' vol. 6.

"Die Metamerie des Kopfes und die Wirbeltheorie des Kopfskeletes," 'Morphol. Jahrb.,' vol. 13.

Jackson and Clarke. "The Cranial Nerves of Echinorhinus spinosus,' 'Journal of Anat.,' vol. 10.

Krause. "Ueber die Doppelnatur des Ganglion ciliare," 'Morphol. Jahrbuch,' vol. 7.

Marshall. "On the Cranial Nerves of Scyllium," 'Quart. Journ. Microsc. Science,' 1881.

"The Segmental Value of the Cranial Nerves," 'Journal of Anatomy,' vol. 16.

Schwalbe. "Das Ganglion oculomotorii," 'Jenaische Zeitschrift,' vol. 13.

Stannius. 'Das Peripherische Nervensystem der Fische,' Rostock, 1849.

van Wijhe. "Ueber d. Mesodermsegmente, and über die Entwicklung der Nerven des Selachier-Kopfes," Amsterdam, 1882.

April 4, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Baron Henry de Worms, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "On the Magnetic Inclination, Force, and Declination in the Caribee Islands, West Indies." By T. E. THORPE, Ph.D., F.R.S. Received March 16, 1889.

The following determinations of the magnetic elements among the Caribees or Windward Islands were made in August, 1886, on the occasion of the Eclipse Expedition of that year to Grenada.

The instruments employed were magnetometer Elliott No. 61, and Dip Circle Dover 83, belonging to the Science and Art Department.

The method of observation was similar to that adopted in the Magnetic Survey of the British Isles for epoch January 1st, 1886, for which these instruments were also employed.

I. ST. GEORGE, GRENADA.

Station: Near the watering-place and close to the edge of the southern shore of the harbour.

Lat. 12° 2′ 52″ N. Long. 61° 44′ 35″ W.

Inclination.

	Needle I.		Needle II.
Aug. 13, 1886 (2.45 to 4.10 P.M.) ..	40° 53·8′	....	40° 55·6′

Horizontal Force.

(*a*) Deflections.

	Temp.		Observed deflections.
Aug. 13, 1886......	29·1°	......	14° 7′ 48·5″
			5° 53′ 41·2″

(*b*) Vibrations.

	Temp.		Time of one vibration.	
Aug. 13, 1886....	32·6°	....	3·0540 secs.	Mean = 3·0538 secs.
			3·0537 „	

$m = 0{\cdot}00103109$. H = 3·1093.

Declination.

(*a*) Geographical Meridian.

	Local appt. time of passage of ⊙ centre. hr. m. s.		⊙ alt.		Correct. for mirror.		Geographical meridian.
Aug. 13, 1886..	4 33 29	....	23° 40′	....	−0·1	....	252° 14′

(*b*) Magnetic Meridian.

	L.M T. hr. m.		Torsion.		Magnetic meridian.
Aug. 13, 1886....	4 14	....	−0·4′	....	252° 55·9′

II. Hog Island, Bay of Clarkes Court.

Station: Site of Eclipse Station; on a creek on the eastern side of the Island.

Lat. 12° 0′ 40″ N. Long. 61° 43′ 45″ W.

Inclination.

	Needle I.		Needle II.
Aug. 25, 1886 (10.30 to 12.10)....	41° 14′	....	41° 14·2′

Horizontal Force.

(*a*) Deflections.

	Temp.		Observed deflections.
Aug. 22, 1886 (11.5 to 11.31)..	29·7°	....	14° 8′ 38·7″
			5° 54′ 13·7″

(*b*) Vibrations.

	Temp.		Time of one vibration.	
Aug. 22, 1886	28·8°	....	3·0573 secs.	mean = 3·0577 secs.
			3·0582 „	

$m = 0{\cdot}00102963$. H = 3·1000.

Declination.

(*a*) Geographical Meridian.

	Local appt. time of passage of ☉ centre. hr. m. s.		☉ alt.		Correct. for mirror.		Geographical meridian.
Aug. 22, 1886..	2 47 27	....	49° 4·0′	....	−0·5	....	19° 58·9′.

(*b*) Magnetic Meridian.

	L.M.T. hr. m.		Torsion.		Magnetic meridian.
Aug. 22, 1886....	10 9	....	+0·1′	....	20° 50·3′

As the island of Grenada is highly volcanic in parts, it is not improbable that the observations may be affected to some extent by local disturbance.

III. Island of Carriacou.

Station: On the shore of the bay on the southern end of the island. Lat. 12° 27′ N. Long. 61° 29′ W.

Horizontal Force.

Vibrations.

	Temp.		Time of one vibration.	
Aug. 23, 1886.. (4 hr. 38 m.)	33·7°	....	3·0723 secs. / 3·0735 „	mean = 3·0729 secs.

$m = [0·00102963]$. $\quad$ H = 3·0771.

Declination.

(*a*) Geographical Meridian.

	Local appt. time of passage of ☉ centre. hr. m. s.		☉ alt.		Correct. for mirror.		Geographical meridian.
Aug. 23, 1886..	3 53 49	....	32° 54′	....	+0·1	....	148° 52·2′

(*b*) Magnetic Meridian.

	L.M.T. hr. m.		Torsion.		Magnetic meridian.
Aug. 23, 1886....	4 27	....	0·0′	....	149° 8·5′

The observations at Carriacou were much interfered with by rain, and no determinations of dip were possible. The moment of the magnet has been assumed to be that determined at Hog Island on the previous day.

The results may be thus summarised:—

Station: Aug., 1886.	Inclination.	Force.		Declination.
		Horizontal.	Total.	
St. George, Grenada.......	40° 54′·7	3·1093	4·1144	0° 41′·5 E.
Hog Island, Grenada......	41 14·1	3·1000	4·1223	0 51 5 E.
Island of Carriacou........	—	3·0771	—	0 16 3 E.

II. "Experiments on the Resistance of Electrolytic Cells." By Capt. H. R. Sankey, R.E. Communicated by W. H. Preece, F.R.S. Received March 21, 1889.

(Abstract.)

It was observed during the course of some experiments on the electrolytic deposition of copper that the resistance of the electrolytic cells employed was greater the lower the current density, and the experiments described in this paper were undertaken to inquire more definitely into the matter.

Many physicists have already observed the same effect, and have ascribed it to a resistance at the junctions of the electrodes with the electrolyte, and called it "transfer" resistance.

In these experiments a prismatic electrolytic cell of triangular cross-section was employed, and the area of the electrodes was equal to that of the cross-section of the liquid. The electrodes experimented with were electrotype copper, lead, zinc, and platinum, and the electrolytes, solutions of $CuSO_4$ of various sp. gr., neutral and acidulated, of $ZnSO_4$, $MgSO_4$, NaCl, Na_2CO_3, dilute H_2SO_4, &c. The electrodes were placed at different distances apart, but in general had an area of 50 square cm.

All the measurements were made by noting the swing of a Thomson's reflecting galvanometer, used as a potentiometer, and standardised before each trial by means of a Clark's cell.

The current was measured by observing the potential difference across a known resistance.

The P.D. of the cell was proportional to the swing of the spot of light.

The counter E.M.F. was obtained by taking the swing on breaking the circuit, the galvanometer being connected across the terminals of the cell; but this swing is *not* proportional to the C.E.M.F. existing in the cell whilst the circuit is completed. Readings were, there-

fore, taken to obtain the fall of the C.E.M.F. on breaking the circuit, so as to obtain the correction to be applied to the reading. This correction was found to vary considerably according to the electrodes and the electrolytes; with acidulated $CuSO_4$ solutions and electrotype copper electrodes it varied from 3 to 15 per cent.

A variety of tests were made to ascertain what degree of dependence could be placed on these measurements of C.E.M.F., because, of course, on them the whole matter rests. Probably the most conclusive of these tests was the measurement of the resistance (by the method employed in these experiments) of an arrangement, consisting of a box of coils and of an electrolytic cell of very large area, whose resistance might be neglected, but which supplied a C.E.M.F. The measured resistances agreed, within 2 per cent., of the resistance unplugged in the box.

The conclusion come to is that the C.E.M.F. was determined with a fair degree of accuracy, sufficient to show the existence of a transfer resistance.

The resistance of the electrolyte *itself* was measured in some cases by finding the P.D. across two cross-sections of the liquid, by means of fine wires dipping into the liquid at a known distance apart. This resistance was found to be (as might be expected) independent of the current density.

Deducting the resistance of the electrolyte as thus obtained from the resistance of the cell gave the "transfer" resistance.

In commencing each trial a current of about 2·7 milliampères was passed through the cell for some time, which was found to increase the resistance of the cell up to a limit depending on the previous history of the electrodes.

The current was then increased by approximately doubling it each time until it reached about 370 milliampères. It was found that as the current increased the resistance diminished, rapidly at first, more slowly afterwards (set A).

After applying the 370 milliampères current, the current was again suddenly reduced to about 2·7 milliampères, and it was found that the resistance had become much smaller, but that it immediately began to increase again, somewhat rapidly at first (set B).

A few minutes afterwards the current was again increased, as in set A, and the resistance was found to diminish as the current increased, but more slowly than in set A (set C). When a current of 370 milliampères was reached, the resistance in both set A and set C were practically equal, and the transfer resistance was small.

The figure shows one of the trials with acidulated $CuSO_4$ solution and lead electrodes. The sudden rise in the resistance (set A) occurred at the moment the cathode became covered with copper. In this figure the thick line shows set A, the thin line set C, and the

dotted lines the resistance of the electrolyte obtained during set A and set C.

A great many trials were made, some few of which are given in the paper. All give evidence of a transfer resistance diminishing as the current increases.

The view is expressed that the "transfer" *resistance is not due to* a non-conducting layer being formed on one or both electrodes, since if such were the case the resistance would increase as the current increases, and would be greater after the application of a strong current than before. It is suggested that this transfer resistance may be due to some molecular interaction at the junction of the electrodes with the electrolyte, offering a greater resistance to weak currents than to strong, and the reduction of *the resistance after the application of a* strong current supports this suggestion, in that the disturbance set up by the strong currents would probably last for some time.

A trial was made with acidulated $CuSO_4$ solution and electro-copper electrodes under identical conditions, with the exception of the area of the electrodes, which was varied. It was found that the transfer resistance per unit area was sensibly the same for same current density.

The effect of temperature was also inquired into, but only to a limited extent. With weak currents the transfer resistance diminished very rapidly as the temperature increased, and at about 70° C. the transfer resistance was very small.

III. "The Ferment Action of Bacteria." By T. LAUDER BRUNTON, M.D., F.R.S., and A. MACFADYEN, M.A., B.Sc. Received March 28, 1889.

[Publication deferred.]

IV. "On the Limit of Solar and Stellar Light in the Ultra-violet Part of the Spectrum." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received March 28, 1889.

[Publication deferred.]

Presents, April 4, 1889.

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Photograph of the portrait of John Evelyn, F.R.S., in the possession of the Royal Society. Dr. Frankland, F.R.S..

April 11, 1889.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Bakerian Lecture was delivered as follows:—

BAKERIAN LECTURE.—"A Magnetic Survey of the British Isles for the Epoch January 1, 1886." By A. W. RÜCKER, M.A., F.R.S., and T. E. THORPE, B.Sc., Ph.D., F.R.S. Received (in abstract) April 11, 1889.

(Abstract.)

Two magnetic surveys of the British Isles have been made previous to that of which an account is given in this paper. The necessary observations were taken between the years 1834–38 and 1857–62, and the results were reduced to the epoch 1842·5 by Sir E. Sabine ('Phil. Trans.,' 1870, p. 265). The stations in these were very irregularly distributed over the area under investigation, the declination was determined at but few places, and the force in the earlier survey was only determined relatively to London.

In the five years 1884–88, both inclusive, the authors have made an

exhaustive survey of the United Kingdom. They have observed at 200 principal and a number of secondary stations, and at all the principal stations, except three or four, all the magnetic elements have been determined.

The two sets of instruments employed have been carefully compared with each other at the Kew Observatory in 1884, 1886, and 1887, and are in remarkably good accord. All the observations were made by the authors except the dip observations at eight stations in Scotland, for which they have to thank Mr. A. P. Laurie, Fellow of King's College, Cambridge.

The chronometers were frequently compared with Greenwich by means of the 10 A.M. time signal, for leave to receive which the authors are much indebted to the good offices of Mr. Preece, F.R.S.

The probable errors of the observations are as follows:—

Declination	$\pm$ 0'·699
Horizontal force	$\pm$ 0·00028 (M.V.)
Dip	$\pm$ 0'·51

In this computation, only declination observations which are in all respects independent are included. The horizontal force observations are also as independent as is possible for nearly simultaneous observations, and the dips compared are those taken with the two needles.

The authors propose the name *isomagnetics* for the class of curves which are drawn through points at which the values of one of the magnetic elements are constant, and in which isogonals, isoclinals, &c., are included.

To determine the form of the isomagnetics, they divided the area of the survey into nine overlapping districts, and found for each a linear formula which connected the value of the element with the latitude and longitude. By means of this formula they calculated the value of the element at points where the lines of longitude corresponding to whole degrees east or west of Greenwich intersect lines of latitude which correspond to half degrees. Where several districts overlap, the mean value was taken. From the values thus obtained at a series of points regularly distributed all over the country, the isomagnetics were approximately determined. The forms of these curves were slightly irregular, and equations were framed to represent smooth curves which passed through their mean directions. These were the equations to the terrestrial or undisturbed isomagnetics.

These are compared with those obtained in the earlier surveys, and the secular change is fully discussed.

The calculated values of the elements are then obtained for every station, and by comparing these with the observed values, the magni-

tude and direction of the disturbing force at each station was determined.

It was found that the Malvern Hills attract the north pole of the needle strongly.

The well-known fact that the difference of the declination at Kew and Greenwich is much greater than the difference of longitude will explain, is found to be connected with a widespread regional magnetic disturbance within the area of which these observatories lie.

Several methods of argument all point to the conclusion that the centre of this disturbance lies between Windsor and Reading, and a little to the north and east of the latter town. Towards this point all the calculated disturbing forces in the neighbourhood converge.

The authors adopt, as a working hypothesis, the view that this attraction is due to the same cause as that observed at the Malverns, viz., the presence of igneous rocks, and they prove that the range of the disturbance extends from Kenilworth to the Channel, and from Salisbury to the North Sea, a total area of about 10,000 square miles.

As the centre is approached the excess of the observed downward vertical force above that given by calculation increases, and it reaches a maximum at Reading close to the point which a study of the horizontal forces had indicated as the centre.

Extending the same method to the rest of the country, though this has not been studied by them in the same detail as south-eastern England, the authors prove (1) that the results obtained on re-visiting the same station indicate that even in disturbed districts, the direction of the disturbing force can in general be determined by a single set of observations to within 15°, and in most cases to within a much smaller limit; (2) that the directions of the disturbing forces were the same when Mr. Welsh surveyed Scotland in 1857 as they are now; (3) that the horizontal disturbing forces tend towards districts in which the vertical disturbing force is a maximum; (4) that certain regions in which crystalline rocks occur display a marked attraction on the needle; (5) that in certain other regions, and notably in lines running respectively from London to the South Wales coal-field and from the Lincolnshire Wolds to the Lake District, though no crystalline rocks appear on the surface, magnetic attractions, similar to that observed near Reading, are in play, which indicate the existence of crystalline rocks at no great depth; (6) that there are in Great Britain five principal regions of the two kinds referred to in (4) and (5), towards which the horizontal disturbing forces act. Their positions may be defined approximately by means of the following lines, which pass through their central parts, viz., (α) the line of the Caledonian Canal; (β) a line somewhat to the west of the basaltic masses in the Western Isles; (γ) a line passing through the Scotch coal-field, in which crystalline basaltic rocks occur; (δ) a line certainly parallel to,

and possibly coincident with, that along which the Jurassic and Liassic strata thin out very rapidly in south-east Yorkshire, and passing thence towards the Cumberland lakes; (*e*) a line the general direction of which coincides with that of the Palæozoic ridge between London and the South Wales coal-field.

The following Paper was read:—

I. "Experiments on the Nutritive Value of Wheat Meal." By A. WYNTER BLYTH. Communicated by Dr. LAUDER BRUNTON, F.R.S. Received March 30, 1889.

A physician, who may be designated as A, undertook to live for twenty-eight days on distilled water and whole meal. Each day a certain quantity of the meal was ground by himself, weighed, and made either into cakes or porridge by means of distilled water.

The excreta were forwarded to me for analysis.

The experiment may be divided into three stages:—(1) A period of eight days, during which the insufficient quantity of 16 ozs. (453·59 grams) of whole meal was taken; (2) a period of fourteen days, during which 20 ozs. (566·98 grams) of whole meal were taken; (3) a period of seven days during which 28 ozs. (793·77 grams) of whole meal were taken.

General Physiological Effects.

The condition of A was carefully tested by Mr. Randall at Mr. Francis Galton's laboratory, before and during these periods.

Condition before the experiment:—

Weight in clothing	129 lbs.
Strength of squeeze (right hand)	67 „
„ (left hand)	60 „
Breathing capacity	198 cub. in.
Distance of reading diamond numerals (right eye)	9 inches.
Distance of reading diamond numerals (left eye)	7 „
Snellen's type, read at 20 feet distance	D 60
Highest audible note (by whistle)	19,000 vib.
Reaction time (sound)	15*
„ (sight)	15*
Error in dividing wire in half	0 p. c.
„ „ in thirds	0 p. c.
Error in degrees in estimating angle 90°	0°
„ „ „ 60°	11°

* Reaction time for sound and sight in hundredths of a second.

At the end of the first period, during which the insufficient quantity of 16 ozs. was taken, there was a loss in weight of 7 lbs., the breathing capacity seemed a little increased, but the tests showed no other marked deviation from the above. During the second period, in which the meal was increased to 20 ozs., there was a farther loss of 3 lbs. During the third period, when 28 ozs. were taken, this loss of weight ceased and a slight gain was recorded.

During the whole twenty-eight days, A suffered, according to his own account, but trifling inconvenience: the bodily functions were regularly performed, the mental capacity unaltered; there was a marked absence of indigestion, the sleep was sound, and there was no deterioration of muscular power. On the other hand there was a marked decrease of sexual power as well as desire. The appearance of A during and at the end of the experiment was not that of perfect health. The features were pinched; there was slight anæmia.

The measurements and tests as determined in Mr. Francis Galton's laboratory were as follows:—

	1st period.	2nd period.	3rd period.	Last day of experiment.
Weight in clothing.....	122 lbs.	119 lbs.	120 lbs.	120½ lbs.
Strength of squeeze, right hand	76 "	65 "	74 "	73 "
Ditto, left hand	68 "	62 "	68 "	65 "
Breathing capacity.....	203 cub. in.	190 cub. in.	198 cub. in.	189 cub. in.
Distance of reading (diamond type), right eye	9 in.	10 in.	10 in.	9 in.
Ditto, left eye	10 "	9 "	9 "	8 "
Snellen's type, read at 20 feet distance	D 60	D 60	D 60	D 60
Highest audible note (whistle)............	19,000 vib.	19,000 vib.	19,000 vib.	19,000 vib.
Reaction time (sound) .	15*	16*	10*	13*
" (sight) ...	13*	13*	10*	10*
Error in dividing wire in half................	0 p. c.	0 p. c.	0 p. c.	
Ditto in thirds	1 "	1 "	1 "	
Error in estimating angle 90°	1	1 "	0	
Ditto 60°	7	10	10	

Analysis of Income and Output.

The whole meal was analysed by ordinary methods, the nitrogen being determined by Kjeldahl's process, the fat in a Soxhlet's apparatus by exhaustion with petroleum ether. The fæces were passed into strong redistilled methyl alcohol, dried, powdered and treated simi-

* Reaction time for sound and sight in hundredths of a second.

larly to the whole meal. The urine was also treated by Kjeldahl for nitrogen, the solid residue by evaporating several 5 c.c. in platinum dishes to dryness, and the phosphoric acid by the volumetric uranium method. The following tables give the results :—

First Period. Insufficient Supply of Whole Meal.

	Whole meal ingested daily.	Daily excretion.		
		Fæces.	Urine.	
	grams.	grams.	grams.	
Dry substance	392·35	40 4	27·62	−324·33
Nitrogen	9·07	1·72	9·57	+2·22
Fat	8·25	2·52	..	−5·73
Ash	6·94	3·88	4·38	+1·32
Phosphoric acid	3·17	1·5	2·03	+0·36
Sulphuric acid	0·27	0·05	1·46	+1·24
Chlorine	..	..	1·05	+1·05

The table shows that 82·6 per cent. of the dry substance was assimilated, of the fat 69 per cent. disappeared, 2·22 grams of nitrogen were excreted in excess of that ingested, there was (practically) phosphoric acid equilibrium, there were more salts excreted than taken in, and there was excretion of sulphur and chlorine, although the water taken as a drink and mixed with the food was distilled, and only a small quantity of unoxidised sulphur could be detected in the flour.

Second Period. Barely sufficient Ingestion of Whole Meal.

	Whole meal ingested daily.	Daily excretion.		
		Fæces.	Urine.	
	grams	grams.	grams.	
Dry substance	490·44	47·5	29·16	−413·78
Nitrogen	11·34	2·02	9·75	+0·43
Fat	10·31	2·29	..	−8·02
Ash	8·67	3·7	3·99	−0·98
Phosphoric acid	3·97	1·91	1·95	−0·11
Sulphuric acid	0·34	0 03	1·71	+1·40
Chlorine	..	..	0·88	+0·88

84·3 per cent. was therefore digested of the dry substance, 77·7 per cent. of the fat had disappeared, there was (practically) nitrogenous

and phosphoric acid equilibrium, and some small retention of salts. There was a daily excretion of sulphur and chlorine, the latter in small amount only.

Third Period. A sufficient Supply of Whole Meal. Arrest of Loss of Weight.

	Whole meal ingested.	Excretion.		
		Fæces.	Urine.	
Dry substance	680·62	78·4	33 60	−574·62
Nitrogen	15·87	2·6	8·39	−4·88
Fat	14·44	9·24	..	−5·20
Ash	12·04	7·9	3·28	−0 86
Phosphoric acid	5·59	3·5	1·93	−0·16
Sulphuric acid	0·47	0·17	1·82	+1·52
Chlorine	..	..	1·06	+1·06

During this last period there was retention of nitrogen. The phosphates were pretty well balanced, that is, ingestion was nearly equal to excretion, 83·6 per cent. of the total dry substance was digested, but only 36 per cent. of the fat. It is to be noted that there was an undiminished urinary output of chlorine and sulphur.

The constant undiminished excretion of sulphuric acid as sulphate by the urine and a small quantity of unoxidised sulphur by the intestinal canal, although only traces were found in the flour itself, rendered it desirable that there should be a control experiment upon some other person. Accordingly, an Oxford graduate, upon whom every reliance could be placed, undertook to live for one week upon whole meal and distilled water. This gentleman will be referred to as O.

O lived a sedentary life, was of a slight build, and weighed 137 lbs. at the commencement of the experiment. He took also each day a measured quantity of olive oil, the oil being mixed with the whole meal and baked with it. The quantity of whole meal taken daily varied from 16—22 ozs. The solid excreta of the last three days only were collected for analysis, and the urine of the last two days.

General Results of the Ingestion of Whole Meal by O.

	Whole meal ingested.	Excretion.		
		Fæces.	Urine.	
Dry substance	497·53	28·88	33·13	*−463·08
Nitrogen	11 33	1·18	10·30	+0·15
Fat	10·31	2·58	..	* −35·24
(Olive oil)	(27·51)	..	..	
Ash	8·67	3·35	3·67	−1·65
Sulphuric acid	0·34	..	0·91	+0·57
Phosphoric acid	3·05	1·42	1·72	+0·09
Chlorine	..	..	0·07	+0·07

O, therefore, digested 88·1 per cent. of dry substance ingested; 93 per cent. of the fat disappeared. There was (practically) nitrogenous and phosphoric acid equilibrium; there was some retention of salts, perhaps to be attributed to the small quantity of liquid O drank. Sulphur was excreted, although only traces were ingested, and the excretion of chlorine was small.

At the end of the experiment O was in good health. He had lost a little in weight—1·25 lbs.

The importance of obtaining exact information of the nutritive powers of bread as the basis of ordinary diet need scarcely be accentuated. The quantities of whole meal consumed *per diem* were, it is obvious, deficient in nitrogen, in fat, and in salts. Both of the gentlemen who undertook the experiment lived an ordinary town life, that is, they daily took moderate exercise, but their pursuits involved no manual or hard labour, and therefore must be classed as sedentary; but a less supply than 18 grams of nitrogen and 5 grams of fat would not be likely to keep either of them for a long period in the highest health. The excretion of sulphate by the urine and of unoxidised sulphur by the bowel is interesting and demands still further experiment; considering that sulphur is an essential component of albumen, too little attention has hitherto been paid to its study as a food, but it is obvious that once it is accepted that the external supplies of sulphur were cut off, the sulphur found must have been derived from sulphur stores in the body, with possibly a trifling amount condensed in the lung from breathing London air.

If the excretion by the bowel be considered waste, then on an average 15·6 per cent. of the total nitrogen in the bread or whole meal is not in an assimilable form; about 37 per cent. of the fat is also not digested, and 51·8 per cent. of the ash also passes away.

* Obtained by subtracting 497·53 + 27·51 (olive oil) from united residue of fæces and urine.

The Society then adjourned over the Easter Recess to Thursday, May 2nd.

Presents, April 11, 1889.

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"Second Series of Results of the Harmonic Analysis of Tidal Observations." Collected by G. H. DARWIN, LL.D., F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge. Received January 18,—Read February 7, 1889.

A collection of results by Major Baird and myself has been already published in the 'Proceedings of the Royal Society,' No. 239, 1885; and the present paper brings together new results which I have been able to collect since the date of that paper. I begin with some remarks on the sources of information, and on the observations at each station. A table of the latitudes and longitudes of the places of observation is prefixed to those of the harmonic constants.

Dover.

In the Second Report of the Committee of the British Association on the "Tides of the English Channel and the North Sea" (1879), the following passage occurs :—

"The importance of an accurate knowledge of the tides at Dover in particular, in connection with those of the entire English Channel, being soon made evident to the Committee, as well as the great advantage which would ensue from the establishment of a self-registering tide-gauge at that place, the matter was brought by the Chairman under the notice of the Board of Trade; the request being further supported by the Lord Warden of the Cinque Ports, Earl Granville. The Board of Trade received the request most favourably, and consented to establish at their own expense a self-registering gauge, at a site some distance down the Admiralty Pier, where a tide-well had been made during the original construction of the pier; its connection with the water outside being at a level twelve feet below the low water of ordinary spring tides. The gauge, embracing Sir William Thomson's latest improvements, has been constructed and erected by Messrs. A. Legé and Co., under the direction of Mr. Edward Druce, C.E., the resident engineer in charge of the Admiralty Works at Dover. It will remain, of course, in the hands of, and under the control of the Board of Trade."

In 1886 another Committee of the British Association, appointed to consider the tides of Dover, exhibited to the meeting the tide-curves for Dover for the four years 1880–83, and it was stated that the Minister of Public Works of Belgium had presented to the Secretary of the Committee copies of the self-registered tide-curves for Ostend for several years. A comparison of the high and low waters at the two ports during one lunation is given in the Report of this Committee.

Mr. J. N. Shoolbred, the Secretary of both Committees, was instructed to intrust the curves to me, in order that they might be submitted to harmonic analysis. He afterwards was so good as to obtain from Mr. Druce the continuation of the Dover curves. As the reduction of the whole series of curves would have been very expensive, it was determined that only the curves for 1883-4-5 should be treated; these years were selected because there was reason to suppose that the curves were more accurate than the earlier ones.

To meet the expense of the reduction, Sir William Thomson obtained £50 from the Royal Society Grant, and this sum was afterwards handed to me. The amount would, however, have been altogether insufficient if Major Baird had not interested himself in the matter, and introduced me to Mr. E. Connor, of the Tidal Department of the Survey of India. Mr. Connor then generously offered to devote his spare time to the work, and undertook the superintendence of the native computers at Poona. The reductions of three years of Dover curves, and of the same three of Ostend curves, have been made with all the thoroughness and care of the Indian work. The computations themselves are now in my hands, and the curves have been returned to Mr. Shoolbred.

The tidal record was frequently interrupted at Dover, for there are 34 days wanting in 1883, 57 days in 1884, and 72 days in 1885. The gaps are only of a few days at a time, except from September 24 to October 26, 1885.

The zero of the Dover gauge is said to be 8·67 feet below the Ordnance datum, and therefore 11·33 feet above the "international datum," which is stated in the British Association Report (1879) on Levels to be 20·00 feet below English Ordnance datum.

The reduction of the tide curves shows that the mean sea level at Dover was, in 1883, 0·52 foot; in 1884, 0·46 foot; and in 1885, 0·21 foot above Ordnance datum.

The French Nivellement Général is 2·625 feet below Atlantic M.S.L., and 1·992 foot below Ordnance datum. Hence Atlantic M.S.L. is 0·633 foot above Ordnance datum. Thus Dover M.S.L. was, in 1883, 0·11 foot; in 1884, 0·17 foot; and in 1885, 0·42 foot below Atlantic M.S.L.

It appears from the Ostend curves that Ostend M.S.L. was, in 1883, 0·25 foot; in 1884, 0·37 foot; and in 1885, 0·21 foot above Ordnance datum, and therefore in 1883, 0·38 foot; in 1884, 0·26 foot; and in 1885, 0·42 foot below Atlantic M.S.L. Thus Ostend M.S.L. was below Dover M.S.L. by 0·27 foot in 1883; by 0·09 foot in 1884; and they were the same in 1885. By reference to the Atlantic M.S.L. we see that by far the larger part of these remarkable oscillations depends on Dover.

But it is nearly incredible that the sea at Dover should have been

as much as $3\frac{3}{4}$ inches lower in 1885 than in 1883, and I do not believe that the numbers are accurate.

This opinion is confirmed by even a casual examination of the results of the harmonic analysis at Dover, the observations being obviously bad; for we may, I think, reject the supposition that both the tide and the mean sea level at Dover are actually far more irregular than at any other port.

In order to test the Dover results, I have found the mean error (according to the method of least squares) of the phases of the several tides from the three years tabulated. I have then rejected as worthless all those tides in which the mean error of phase amounts to 30°. By this criterion the tides S_1, S_4, S_6, S_8, K_2, J, Q, T, 2SM, and all the tides of long period are rejected, and many of those retained will be seen to be really very bad.

Thus the mean error of phase of M_2 is 7°·3, and of S_2, 9°·5. The physical meaning of this is, that it is an even chance that the principal lunar high water occurs within a specified 20 minutes of time, and that the principal solar high water occurs within a specified 25 minutes. With fairly good observations these periods should, from three years of observation, be about 4 or 5 minutes for the lunar tide, and 8 or 10 minutes for the solar tide. In the case of the tides at New York, tabulated below for three years, it is an even chance that lunar high water occurs within a specified $1\frac{1}{2}$ minutes, and solar high water within a specified $6\frac{1}{2}$ minutes.

The Ostend results were treated in the same way as the Dover ones, and compare very favourably with them, although not, I think, of the highest order of perfection.

It may thus be safely concluded that the observations at Dover have been very badly made.*

It is a pity that an expensive instrument should have been installed, and that its records for many years should be rendered valueless by the want of proper supervision.

I publish the results, however, for what they are worth.

The phases of the several tides are referred to Greenwich time.

Ostend.

I have no information as to the manner in which these observations were taken, but, as stated above, the curves were presented by the Minister of Public Works of Belgium. The Ostend M.S.L. was stated in considering the Dover curves. The zero of the tide gauge is 8·17 feet above the international datum. There were many interrup-

* Captain Wharton, R.N., is of opinion that the situation of Dover is such that the tides are likely to be irregular there. I cannot, however, believe that this affords a sufficient explanation of the irregularity of the results.—May 8, 1889.

tions in the working of the gauge, the gaps being 64 days in 1883, 64 days in 1884, and 14 days in 1885.

It has already been remarked that the Ostend observations were apparently well made, although, perhaps, not of the very highest perfection.

The results are referred to Ostend local time.

Heligoland.

The results for Heligoland are taken from Dr. Börgen's paper on the Tides of South Georgia and Kingua-Fjord,* where they are given incidentally as a means of testing a proposed method of reduction. The observations appear to have been made in 1882, and the reductions were, I believe, made by Dr. Börgen. The heights were given in centimetres, but have been reduced to feet.

Copenhagen, Nanortalik, Angmagsalik, Godthaab.

I owe these observations to Dr. Crone, of Copenhagen, by whom, I believe, the reductions were performed.

The observations at Nanortalik and Angmagsalik were made by a Danish Expedition between 1883 and 1885. At the latter station the observations were very short, and Dr. Crone has only attempted to determine the mean lunar interval of 4 h. 6 m., or κ of M_2.

The heights were given in centimetres, but have been reduced to feet.

The observations at Godthaab were made by the Danish Polar Expedition of 1882–3; they extended from July 16 to August 31, 1883.

Dr. Crone has written a paper entitled "Flux et Reflux de la Mer à Godthaab."

South Georgia and Kingua-Fjord.

These observations were made by the Arctic and Antarctic expeditions of the German Government. The observations in South Georgia were made with a self-registering tide-gauge, those at Kingua-Fjord by the officers of the ship. The observations were reduced by Dr. Börgen, of Wilhelmshaven, and further information will be found in the paper referred to above.

The gauge was erected in South Georgia in January, 1883, and was in operation until the end of April, when it was put out of order by heavy weather. The observations began again on 21st May, and continued until 2nd September, with breaks of only a few hours or of a day caused by ice. The means of the values derived from the two periods of observation are given below.

* 'Separat-Abdruck aus dem Deutschen Polarwerke,' Asher, Berlin.

At Kingua-Fjord, the head of the expedition, Dr. Giese, charged M. Mühleisen with the duty of making the observations. The observations began on 22nd July at 6 A.M., and continued until 1st September, 8 P.M., a period of 41 days. The height of water was observed every two hours, and also every five minutes about high and low water. From these observations a continuous tide-curve was formed which was treated by harmonic analysis.

Dr. Börgen informs me that the values of κ for the diurnal tides K_1, O, P, as printed in his paper, require correction by 180°. This arose from the fact that the observations, as subjected to reduction, began at midnight. The correction has been made in the table below. The heights are given in metres by Dr. Börgen, but have been reduced to feet.

Kerguelen Island.

These results are from a letter of Dr. Börgen to me, dated July 22, 1887. He writes:—

"I have just finished the calculation of the tides at Kerguelen Island, Betsy Cove, where we had a self-registering tide-gauge put up by the officers of H.M.S. "Gazelle," when there for the purpose of observing the transit of Venus in 1874. The observations commence at noon November 16, 1874, and close at noon January 29, 1875. Some difficulties, which arose from choking up and partially destroying the pipe in which the float moved, caused two interruptions of five and nine days. From this cause, and because the weather in that region is rather boisterous (we noticed 450 hours out of a quarter of a year, or 2,160 hours, with a velocity of the wind higher than 15 metres per second), I am inclined to think the constants are not quite so satisfactory as they would have been in a calmer ocean."

The results have been reduced from centimetres to feet.

The Hudson Straits Stations.

The observations at these stations were taken under the supervision of Lieutenant Gordon, R.N. The length of observation at each station was short, and the results must be correspondingly uncertain. The dates at which the observations began are entered in the table below, together with the periods.

The observations at Port Burwell were taken every two hours, and at all the other stations, besides the bi-hourly measures, observations were taken at intervals of five minutes about the times of high and low water. The reductions were made by Lieutenant Gordon, with the assistance of Professor Carpmael, of Toronto.

During the observations at Ashe Inlet, and at Stupart's Bay, the Straits were choked with ice, and this may have exercised some influence on the tides.

Governor's Island, New York Harbour.

In an appendix to the 'Report of the United States Coast Survey' for 1885, Professor Ferrel gives the results of harmonic analysis applied to tidal observations at this station. A map shows the sites of the tide-gauges at Governor's Island and at Sandy Hook.

Mr. Ferrel's treatment of the tide M_1 differs from that recommended in the Reports of the British Association, and his entry for M_1 is therefore here omitted.

In the preface to the previous collection of results a memorandum by Mr. Ferrel, about the phases of the tides, was quoted. In a footnote,* added after the paper had been presented, I remarked that it was not easy to accept Mr. Ferrel's memorandum as conclusive of the identity of treatment of the American tides with the procedure recommended by the British Association. The same reason, which then caused me to feel this doubt, applies to the present series of results, and it will therefore be well to state the case somewhat more fully than was possible in the footnote referred to.

In the 'British Association Report for 1883' the equilibrium theory of tides is developed so that each tide is represented by a *positive* cosine. Now, there are two of the tides, viz., those initialled L and λ, in which the development naturally leads to a *negative* cosine, and if these terms are to appear as positive cosines, 180° must be added to the argument. It follows, therefore, that if Mr. Ferrel retains the cosines in the negative form, the angles κ for L and λ, as tabulated by him, must be augmented by 180°, in order to bring his results into accordance with ours. Now, it may be observed that in all the results tabulated by the U.S. Coast Survey, the tides L and λ are apparently in diametrically the opposite phase from that of all the other semidiurnal tides.

That this is actually the case appears physically so improbable that I conjecture, even in the face of Mr. Ferrel's memorandum, that he uses a different convention as to the tides L and λ, and that to read his results in our notation his values of κ should be augmented by 180°. I here tabulate, however, the values as I find them.

Whilst speaking of this point, it is impossible not to refer to the very remarkable peculiarity of the tide K_2 in the results for Sandy Hook in the previous collection, and for Governor's Island here. It is obvious that all the semidiurnal tides of true astronomical origin should be nearly in the same phase, but here we have a single tide exactly inverted as compared with the rest. Is it possible that by some accidental change of sign 180° can have been erroneously imported into the result?

Singapore and Hongkong.

I have no information about these observations. The results were, however, kindly placed at my disposal for this collection by Mr. Roberts. They were given me in the form which was used before the publication of the Report of 1883 to the British Association, and I am responsible for the reduction to the standard form.

Mr. Roberts performed the reductions of the observations himself, and has published the tide tables for the two ports on behalf of the Governments of the two colonies. He proposes to write a paper on these tides, which will doubtless give the information which is here wanting.

Indian Stations.

Major Baird and Mr. Connor have sent me for publication the values of the constants at a large number of stations in India.

I have divided them into two groups. The first of these comprises stations for which results were published in the paper by Major Baird and myself in the 'Proceedings of the Royal Society.' Many years of observation are thus added to the previous ones, and the mean values of the constants given below include the values given in our paper of 1885. The station at Karachi is especially valuable for tidal theory, since we now have results for nearly a whole lunar cycle of nineteen years. The second group comprises a number of ports, for which the constants have been only hitherto published in the prefaces to the Indian Tide Tables.*

The constants for certain tides initialled 2N, MN, MK, 2MK are now given for the first time.† The first of these, 2N, is the elliptic semidiurnal tide of the second order. It appeared from the development of the equilibrium theory that it might be easily sensible, and the values now given prove that this is the case. The other three, MN, MK, 2MK, are shallow water tides arising from the interference of the principal lunar tide M_2, 1st, with the larger elliptic tide N, 2ndly, with the luni-solar diurnal tide K_1, and 3rdly, with the lunar diurnal tide O. The two latter of these, viz., MK and 2MK, also arise from the interference of M_4 with O, and from M_4 with K_1. The values appear to be all fairly consistent from year to year at the riverain stations, but at other places they are obviously quite without significance.

Mean Sea Levels.

In our previous paper we did not give the mean sea levels, as determined from each year of observation.

* Published by authority of the Government of India.

† See introduction to our previous paper on the "Results of Harmonic Analysis."

Major Baird has now caused to be sent the mean sea levels with reference to the zeros of the several tide-gauges. The reference of the zero of any gauge to a bench-mark ashore has principally a local interest. Full statements on this head are given in the prefaces to the Indian Tide Tables, but those are not reproduced.

The table of mean sea levels which follows immediately comprises all the stations in which more than a single year of observation has been reduced. The day of the month, prefixed to each series of results, denotes the first day of the year for which the mean sea level is given.

In the Fourth Report to the British Association on 'Harmonic Analysis' (1886), it is shown that the oscillations of mean sea level are far too large to be explained by the known astronomical inequality with a period of nearly nineteen years.

This is not a convenient occasion for the discussion of the present series of values, but I remark that 1882 was a year in which the whole Indian Ocean stood low, whilst 1885 was one in which it stood high.

If variation in the Sun's temperature is the cause of variation of sea level, we might expect to find a periodicity with a period of ten or eleven years. It is then worth noticing that at Karachi there is a minimum in 1872 and again in 1882.* The observations are clearly insufficient to do more than to raise the question.

[Captain Wharton has been good enough to give me Mr. Russell's results for mean sea level at Sydney, and it is interesting to note the very large oscillation of level, with a minimum simultaneous with that at Karachi.]†

* Spörer gives 1878·8 as the time of minimum sun-spots.

† May 8, 1889.

Height in feet of Mean Sea-level above Zero of Gauge.

Aden.

(March 3.)

1879–80	5·767
1880–1	·784
1881–2	·814
1882–3	·754
1883–4	·800
1884–5	·849
1885–6	·883
1886–7	·902

Karachi.

(May 1.)

1868–9	7·149
1869–70	·291
1870–1	·264
1871–2	·107
1872–3	·051
1873–4	·079
1874–5	·152
1875–6	·158
1876–7	·134
1877–8	·207
1878–9	·331
1879–80	·308
1880–1	·267
1881–2	·179
1882–3	·060
1883–4	·192
1884–5	·198
1885–6	·206

Bhavnagar.

(January 1.)

1886	22·799
1887	·710

Bombay.

(January 1.)

1878	10·265
1879	·184
1880	·187
1881	·248
1882	·194
1883	·257
1884	·256
1885	·304
1886	·267

Mormugão.

(March 16.)

1884–5	5·512
1885–6	·577
1886–7	·578

Karwar.

(March 1.)

1878–9	5·650
1879–80	·541
1880–1	·564
1881–2	·515
1882–3	·492

Beypore.

(December 1.)

1878–9	5·385
1879–80	·392
1880–1	·412
1881–2	·412
1882–3	·395
1883–4	·301

Cochin.

(January 25.)

1886–7	2·422
1887–8	·359

Galle.

(April 1.)

1884–5	2·656
1885–6	·700
1886–7	·679

Colombo.

(February 1.)

1884–5	2·208
1885–6	·261
1886–7	·304

Paumben.

(October 1.)

1878–9	2·666
1879–80	·707
1880–1	·759
1881–2	·705

Negapatam.

(December 6.)

1881–2	1·996
1882–3	2·048

(March 20.)

1885–6	1·811
1886–7	2·048
1887–8	2·047

Port Blair.

(April 19.)

1880–1	4·792
1881–2	·718
1882–3	·710
1883–4	·726
1884–5	·689
1885–6	·612
1886–7	·506

Moulmein.

(April 17.)

1880–1	8·453
1881–2	·659
1882–3	·638
1883–4	·737
1884–5	·146
1885–6	·888

Amherst.

(August 5.)

1880–1	13·591
1881–2	·974
1882–3	·701
1883–4	·767
1884–5	·588
1885–6	·811

Rangoon.

(March 1.)

1880–1	15·074
1881–2	14·980
1882–3	·958
1883–4	·925
1884–5	·739

Elephant Point, New Site.

(January 1.)

1884	....	16·314
1885	....	15·641
1886	....	·878
1887	....	·799

Chittagong.

(June 6.)

1886–7	....	8·251
1887–8	....	7·945

Kidderpore.

(March 22.)

1881–2	....	10·739
1882–3	....	·686
1883–4	....	·599
1884–5	....	·669
1885–6	....	·950

Diamond Harbour.

(April 4.)

1881–2	....	8·976
1882–3	....	9·011
1883–4	....	8·999
1884–5	....	·897
1885–6	....	·804

Dublat.

(April 22.)

1881–2	....	14·394
1882–3	....	·499
1883–4	....	·417
1884–5	....	·379
1885–6	....	·263

False Point.

(May 1.)

1881–2	....	7·552
1882–3	....	·597
1883–4	....	·593
1884–5	....	·492

Vizagapatam.

(February 8.)

1879–80	....	4·991
1880–1	....	·917
1881–2	....	·809
1882–3	....	·812
1883–4	....	·813
1884–5	....	·680

Cocanada.

(March 31.)

1886–7	....	5·488
1887–8	....	·212

Madras.

(February 1.)

1880–1	....	2·251
1881–2	....	·209
1882–3	....	·179
1883–4	....	·180
1884–5	....	·134
1885–6	....	·051

Sydney Harbour.

(January 1.)

1873	....	3·531
1874	....	·623
1875	....	·566
1876	....	·502
1877	....	·367
1878	....	·293
1879	....	·247
1880	....	·100
1881	....	2·550
1882	....	·507
1883	....	·563
1884	....	·579
1885	....	·453

Table of Latitudes and Longitudes.

European Stations.

	lat.		long.
Dover	51° 7′ N.	....	1° 9′ E.
Ostend	51 14	....	2 55
Heligoland	54 48	....	7 50
Copenhagen	55 14	....	12 35

Greenland and Davis Straits.

Angmagsalik	65 37 N.	....	37 15 W.
Nanortalik	60 8	....	45 16
Godthaab	64 12	....	51 44
Kingua Fjord	66 36	....	67 20

Hudson's Straits.

Port Burwell	60 25 N.	....	64 46 W.
Ashe Inlet	62 33	....	70 35
Stupart's Bay	61 35	....	71 32
Nottingham Island	63 12	....	77 28
Port Laperrière	62 34	....	78 1

Southern Stations.

Kerguelen Island, Betsy Cove	49	9 S.	70	12 E.
South Georgia	54	31	36	1 W.

U.S. Coast Survey.

Governor's Island, New York Harbour	40	42 N.	74	1 W.

Straits Settlement and China.

Singapore	1	17 N.	103	51 E.
Hong Kong	22	16	114	10

Old Indian Stations.

Aden	12	47 N.	44	59 E.
Karachi	24	47	66	58
Bombay	18	55	72	50
Beypore	11	10	75	49
Negapatam	10	46	79	53
Madras	13	4	80	15
Vizagapatam	17	41	83	17
False Point	20	25	86	47
Dublat	21	38	88	6
Diamond Harbour	22	11	88	14
Kidderpore	22	32	88	22
Rangoon	16	46	96	12
Amherst	16	5	97	34
Moulmein	16	29	97	40
Port Blair	11	41	92	45

New Indian Stations.

Bhavnagar	21	48 N.	72	9 E.
Mormugão	15	25	73	50
Cochin	9	58	76	15
Galle	6	1	80	13
Colombo	6	56	79	50
Cocanada	16	56	82	15
Chittagong	22	20	91	50
Akyab	20	8	92	57
Elephant Point, New Site	16	29	96	19

I.—Table of Harmonic Constants at various Ports.

Dover.

Commence 0 h., January 1.

Year		1883.	1884.	1885.	Mean.	Mean error of phase.
S_2	$H =$	2·42	2·09	1·70	2·066	9°·5
	$\kappa =$	17	22	39	26	
M_2	$H =$	7·54	7·43	6·64	7·202	7°·3
	$\kappa =$	328	329	344	334	
M_3	$H =$	0·05	0·05	0·005	0·036	9°
	$\kappa =$	35	41	57	45	
M_4	$H =$	0·84	0·84	0·55	0·743	11°
	$\kappa =$	214	218	240	224	
M_6	$H =$	0·219	0·20	0·10	0·172	5°·1
	$\kappa =$	89	93	101	94	
M_8	$H =$	0·08	0·08	0·06	0·069	5°·4
	$\kappa =$	1	1	349	357	
O	$H =$	0·17	0·19	0·19	0·188	4°·3
	$\kappa =$	183	182	191	185	
K_1	$H =$	0·18	0·15	0·14	0·140	10°
	$\kappa =$	52	32	55	46	
P	$H =$	0·07	0·05	0·03	0·050	12°
	$\kappa =$	31	3	26	20	
L	$H =$	0·42	0·36	0·35	0·374	25°
	$\kappa =$	26	326	342	351	
N	$H =$	1·54	1·45	1·07	1·357	6°·5
	$\kappa =$	321	309	324	318	
2N	$H =$	0·24	0·28	0·18	0·233	2°·6
	$\kappa =$	279	278	273	276	
ν	$H =$	0·43	0·34	0·40	0·390	12°
	$\kappa =$	280	305	278	288	
μ	$H =$	0·38	0·43	0·41	0·407	24°
	$\kappa =$	35	62	93	64	
MS	$H =$	0·53	0·48	0·34	0·452	18°
	$\kappa =$	270	276	311	286	

I.—Table of Harmonic Constants at various Ports.

Ostend.

Commence 0 h., January 1.

Year		1883.	1884.	1885.	Mean.	Mean error of phase.
S_1	H =	0·056	0·092	0·053	0·067	15°
	κ =	292	317	280	297	
S_2	H =	1·638	2·030	1·720	1·796	4°·9
	κ =	65	57	69	63	
M_2	H =	5·858	6·004	5·889	5·917	0°·5
	κ =	12	12	13	12	
M_3	H =	0·016	0·018	0·031	0·020	13°
	κ =	77	62	93	77	
M_4	H =	0·342	0·383	0·367	0·364	1°·4
	κ =	344	345	347	345	
M_6	H =	0·213	0·256	0·228	0·232	1°·9
	κ =	316	312	316	314	
M_8	H =	0·090	0·117	0·111	0·106	8°·9
	κ =	243	237	247	242	
O	H =	0·326	0·321	0·322	0·323	8°·4
	κ =	174	169	177	173	
K_1	H =	0·167	0·177	0·183	0·176	1°·2
	κ =	354	352	355	354	
P	H =	0·105	0·050	0·081	0·079	9°·4
	κ =	342	320	335	332	
Q	H =	0·088	0·135	0·117	0·113	6°·4
	κ =	127	142	130	133	
L	H =	0·687	0·510	0·325	0·507	19°
	κ =	35	79	48	54	
N	H =	0·945	1·172	0·876	0·998	6°·9
	κ =	6	5	351	0	
ν	H =	0·336	0·468	0·239	0·348	21°
	κ =	340	320	10	343	
MS	H =	0·233	0·245	0·223	0·234	5°·6
	κ =	54	45	59	53	
2SM	H =	0·155	0·127	0·160	0·114	30°
	κ =	291	359	298	316	
Mf	H =	0·177	0·210	0·134	0·174	28°
	κ =	115	135	68	106	
Sa	H =	0·166	0·098	0·219	0·161	23°
	κ =	205	255	207	222	

I.—Table of Harmonic Constants at various Ports.

Year	Heligoland, 1882.	Copenhagen.	Greenland. Angmagsalik.	Greenland. Nanortalik.	Davis Straits. Godthaab, 16 July to 31 Aug., 1883.	Davis Straits. Kinguafjord, 1883 (6 weeks).
S_2 H =	0·79	0·089	....	1·24	1·54	2·67
S_2 κ =	40	249	....	203	229	202
S_4 H =	....	.....	....	....	....	Small
S_4 κ =						
M_2 H =	8·10	0·196	....	2·88	4·46	7·43
M_2 κ =	333	277	119	161	193	159
M_4 H =	....	.....	....	....	....	Small
M_4 κ =						
O H =	0·24	0·069	....	0·36	0·30	0·88
O κ =	243	9	....	74	81	47
K_1 H =	0·21	0·376	....	0·62	0·69	0·27
K_1 κ =	35	23	....	114	127	32
K_2 H =	0·17	0·016	....	....	0·43	0·76
K_2 κ =	27	245	....	....	227	199
P H =	0·09	0·011	....	....	0·23	0·84
P κ =	53	.....	....	....	125	38
L H =	0·46	0·022	....	....	0·13	0·16
L κ =	342	48	....	....	291	167
N H =	0·48	0·056	....	....	0·86	1·20
N κ =	299	248	....	....	188	144

I.—Table of Harmonic Constants at various Ports.

Year	Hudson's Straits.					South Georgia, 1883 (Jan. to Sept. 2, except 3 weeks).	Kerguelen Island, Nov. 16, 1874, to Jan. 29, 1875.
	Port Burwell, 1885 (2 weeks).	Ashe Inlet, 1886 (month).	Stupart's Bay, 1886 (2 weeks).	Nottingham Island, 1886 (month).	Port Laperrière, 1886 (2 weeks).		
S_2 H =	2·33	3·98	3·05	1·77	1·24	0·88	0·80
S_2 κ =	305	296	289	321	316	236	52
S_4 H =	....	....	....	....	....	0·004	
S_4 κ =	....	....	....	....	....	39	
M_2 H =	7·12	11·00	9·02	4·74	3·09	0·74	1·42
M_2 κ =	263	234	227	260	257	213	9
M_4 H =	....	....	....	....	....	0·01	0·03
M_4 κ =	....	....	....	....	....	308	289
O H =	0·19	0·21	0·31	0·25	0·04	0·33	0·22
O κ =	157	349	6	17	126	18	292
K_1 H =	0·48	0·52	0·47	0·22	0·14	0·17	0·14
K_1 κ =	114	108	103	91	64	52	289
K_2 H =	0·64	1·08	0·83	0·48	0·34	0·11	0·23
K_2 κ =	305	296	289	321	316	233	49
P H =	0·16	0·17	0·16	0·07	0·05	0·05	0·045
P κ =	114	108	103	91	64	50	287
L H =	....	....	....	....	....	0·04	0·045
L κ =	....	....	....	....	....	209	50
N H =	....	....	....	....	....	0·16	0·24
N κ =	....	....	....	....	....	199	330

I.—Table of Harmonic Constants at various Ports.

	Governor's Island, New York Harbour.					*Singapore.*	*Hongkong.*
Year......	1876.	1877.	1878.	Mean.	Year......	October, 1882 (1 year).	1883 (1 year).
S_1 { H =	0·033	0·045	0·050	0·042	S_1 { H =	0·053	0·04
κ =	242	223	238	234	κ =	211	101
S_2 { H =	0·408	0·416	0·427	0·417	S_2 { H =	1·067	0·56
κ =	255	256	261	257	κ =	348	292
S_3 { H =	0·045	0·037	0·043	0·042	M_2 { H =	2·602	1·43
κ =	99	87	87	91	κ =	300	266
S_4 { H =	0·036	0·051	0·036	0·041	M_4 { H =	0·053	0·08
κ =	71	61	80	70	κ =	264	320
M_2 { H =	2·153	2·147	2·152	2·149	M_6 { H =	0·035	0·01
κ =	231·8	230·5	230·6	231·0	κ =	43	113
M_3 { H =	0·023	0·020	0·018	0·023	O { H =	0·918	0·86
κ =	210	206	189	202	κ =	53	248
M_4 { H =	0·084	0·075	0·086	0·082	K_1 { H =	0·949	1 19
κ =	334	329	328	330	κ =	100	297
M_6 { H =	0·066	0·066	0·071	0 068	K_2 { H =	0·318	0·16
κ =	90	85	82	86	κ =	345	289
O { H =	0·163	0·150	0·156	0·156	P { H =	0·291	0·38
κ =	109	100	101	103	κ =	93	285
K_1 { H =	0·317	0·322	0·322	0·320	J { H =	0 037	0·02
κ =	106	106	106	106	κ =	115	233
*K_2 { H =	0·129	0·118	0·114	0·120	Q { H =	0·190	0·14
κ =	67	52	37	52	κ =	16	232
P { H =	0·107	0·115	0·093	0·105	L { H =	0·197	0·04
κ =	103	106	104	104	κ =	310	264
N { H =	0·461	0·482	0·497	0·480	N { H =	0·452	0·26
κ =	211	207	211	209	κ =	272	255
*L { H =	0·100	0·114	0·096	0·103	ν { H =	0·058	0·11
κ =	64	67	52	61	κ =	226	290
ν { H =	.....	.....	0·155		μ { H =	0·051	0·07
κ =	.....	.....	203		κ =	97	239
					Sa { H =	0·308	0·485
					κ =	209	226
					Ssa { H =	0·312	0·10
					κ =	234	90

* See remarks in preface on the phases in these cases.

II.—Table of Harmonic Constants at Old Indian Ports.

Aden.

Commence 0 h., March 3.

Year		1883–4.	1884–5.	1885–6.	1886–7.	Mean of 8 years.
S_1	H =	0·094	0·074	0·077	0·070	0·084
	κ =	165	174	162	171	165
S_2	H =	0·702	0·700	0·692	0·700	0·698
	κ =	245	245	245	247	247
S_4	H =	0·004	0·004	0·005	0·004	0·005
	κ =	244	7	324	318	292
S_6	H =	0·006	0·006	0·005	0·008	0·005
	κ =	185	188	221	214	202
S_8	H =	0·001	0·001	0·002	0·001	0·001
	κ =	222	266	335	340	275
M_1	H =	0·066	0·084	0·015	0·036	0·048
	κ =	31	36	58	97	38
M_2	H =	1·588	1·581	1·573	1·570	1·573
	κ =	225	225	226	227	227
M_3	H =	0·019	0·014	0·021	0·019	0·018
	κ =	205	212	226	219	212
M_4	H =	0·004	0·003	0·008	0·006	0·006
	κ =	346	326	339	332	325
M_6	H =	0·006	0·005	0·003	0·005	0·006
	κ =	358	317	14	350	345
M_8	H =	0·003	0·001	0·002	0·003	0·002
	κ =	146	84	21	114	67
O	H =	0·680	0·670	0·669	0·666	0·660
	κ =	38	37	37	37	38
K_1	H =	1·312	1·308	1·307	1·301	1·302
	κ =	34	34	35	36	36
K_2	H =	0·215	0·206	0·195	0·213	0·204
	κ =	234	234	246	244	242
P	H =	0·384	0·399	0·409	0·391	0·392
	κ =	31	32	32	31	32
J	H =	0·131	0·099	0·067	0·087	0·099
	κ =	39	57	45	28	47
Q	H =	0·158	0·144	0·136	0·147	0·149
	κ =	40	29	35	43	39
L	H =	0·028	0·047	0·034	0·048	0·043
	κ =	194	224	197	229	221

II.—Table of Harmonic Constants at Old Indian Ports.

Aden.

Commence 0 h., March 3.

Year		1883–4.	1884–5.	1885–6.	1886–7.	Mean of 8 years.*
N	H =	0·423	0·434	0·444	0·428	0·430
	κ =	217	217	220	221	222
2N	H =	0·087	0·107	0·091	0·067	0·084
	κ =	188	177	199	194	192
λ	H =	0·015	0·037	0·033	.....	0·027 (7)
	κ =	135	259	201	.....	198
ν	H =	0·139	0·156	0·090	0·007	0·099
	κ =	254	214	180	235	223
μ	H =	0·081	0·083	0·080	0·056	0·075
	κ =	193	193	180	194	193
R	H =	.....	0·019	.....	.....	0·009 (3)
	κ =	.....	242	.....	.....	341
T	H =	.....	0·081	.....	0·027	0·052 (4)
	κ =	.....	275	.....	174	232
MS	H =	0·012	0·014	0·006	0·011	0·011
	κ =	138	131	173	146	153
2SM	H =	0·022	0·014	0·019	0·024	0·022
	κ =	107	108	109	109	108
MN	H =	0·044	0·036	0·065	0·031	0·043
	κ =	72	335	37	50	31
MK	H =	0·034	0·033	0·011	0·021	0·024
	κ =	338	43	136	268	289
2MK	H =	0·007	0·008	0·003	0·001	0·008
	κ =	309	282	322	106	5
Mm	H =	0·015	0·039	0·016	0·037	0·035
	κ =	58	53	1	70	20
Mf	H =	0·065	0·012	0·038	0·065	0·045
	κ =	16	36	14	10	25
MSf	H =	0·012	0·019	0·013	0·015	0·014
	κ =	231	265	189	110	225
Sa	H =	0·363	0·367	0·448	0·403	0·392
	κ =	346	356	3	11	358
Ssa	H =	0·114	0·102	0·183	0·166	0·118
	κ =	123	159	144	147	135

* Except where noted thus (4), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Karachi.

Commence 0 h., May 1.

Year ……		1883–4.	1884–5.	1885–6.	Mean of 18 years.*
S_1	H =	0·074	0·065	0·072	0·079
	κ =	171	183	174	161
S_2	H =	0·952	0·963	0·950	0·949
	κ =	324	323	322	322
S_4	H =	0·010	0·011	0·010	0·010 (16)
	κ =	25	44	43	18
S_6	H =	0·006	0·005	0·006	0·007 (15)
	κ =	280	324	316	298
S_8	H =	0·002	0·001	0·001	0 001 (13)
	κ =	288	240	194	213
M_1	H =	0·081	0·042	0·037	0·045 (17)
	κ =	31	111	134	41
M_2	H =	2·566	2·546	2·552	2·518
	κ =	294	294	293	294
M_3	H =	0·029	0·027	0·036	0·038
	κ =	347	349	337	332
M_4	H =	0·033	0·029	0·029	0·025
	κ =	16	21	15	15
M_6	H =	0·050	0·045	0·053	0·049
	κ =	206	206	199	209
M_8	H =	0·005	0·001	0·005	0·005 (15)
	κ =	196	322	267	266
O	H =	0·662	0·666	0·663	0·650
	κ =	48	47	47	47
K_1	H =	1·301	1·300	1·305	1·284
	κ =	47	46	46	46
K_2	H =	0·304	0·308	0·269	0·281
	κ =	322	316	316	319
P	H =	0·392	0·395	0·407	0·383
	κ =	48	46	45	45
J	H =	0·111	0·071	0 040	0·078
	κ =	58	80	46	69
Q	H =	0·133	0·111	0·125	0·128
	κ =	43	46	53	52
L	H =	0·063	0·076	0·075	0·078
	κ =	285	316	281	298

* Except where noted thus (15), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Karachi.

Commence 0 h., May 1.

Year		1883-4.	1884-5.	1885-6.	Mean of 18 years.*
N	H =	0·588	0·596	0·623	0·600
	κ =	278	275	276	277
2N	H =	0·110	0·084	0·109	0·095 (5)
	κ =	241	231	238	247
λ	H =	0·006	0·065	0·066	0·042
	κ =	282	290	241	280
ν	H =	0·028	0·179	0·208	0·141
	κ =	331	320	288	283
μ	H =	0·004	0·041	0·084	0·062
	κ =	276	288	272	266
R	H =	.....	0·010	.....	0·029 (8)
	κ =	.....	312	.....	281
T	H =	.....	0·126	.....	0·075 (8)
	κ =	.....	321	.....	331
MS	H =	0·032	0·025	0·035	0·028 (17)
	κ =	336	339	345	313
2SM	H =	0·028	0·017	0·020	0·021 (13)
	κ =	91	113	125	120
MN	H =	0·040	0·067	0·099	0·069 (5)
	κ =	50	42	31	47
MK	H =	0·068	0·020	0·024	0·042 (5)
	κ =	105	154	358	65
2MK	H =	0·028	0·023	0·019	0·023 (5)
	κ =	23	7	352	15
Mm	H =	0·022	0·027	0·064	0·055 (15)
	κ =	39	119	1	86
Mf	H =	0·061	0·058	0·076	0·039 (15)
	κ =	341	34	122	334
MSf	H =	0·012	0·087	0·064	0·036 (15)
	κ =	138	197	336	258
Sa	H =	0·089	0·139	0·224	0·140 (15)
	κ =	39	44	106	76
Ssa	H =	0·189	0·137	0·109	0·137 (15)
	κ =	170	161	150	146

* Except where noted thus (15), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Bombay.

Commence 0 h., January 1.

Year		1883.	1884.	1885.	1886.	Mean of 9 years.
S_1	H =	0·057	0·059	0·053	0·059	0·069
	κ =	165	173	168	186	178
S_2	H =	1·623	1·636	1·627	1·628	1·625
	κ =	2	1	3	3	3
S_4	H =	0·008	0·007	0·010	0·011	0·010
	κ =	5	359	325	252	287
S_6	H =	0·004	0·003	0·003	0·003	0·003
	κ =	193	169	184	260	185
S_8	H =	0·001	0·003	0·002	0·002	0·002
	κ =	54	124	106	108	107
M_1	H =	0·067	0·125	0·050	0·008	0·056
	κ =	77	55	69	275	40
M_2	H =	4·037	4·071	4·072	4·041	4·043
	κ =	329	328	330	330	330
M_3	H =	0·061	0·064	0·079	0·079	0·067
	κ =	25	25	34	25	25
M_4	H =	0·134	0·126	0·121	0·140	0·127
	κ =	326	320	327	324	323
M_6	H =	0·012	0·011	0·010	0·006	0·011
	κ =	83	58	96	51	94
M_8	H =	0·007	0·008	0·007	0·005	0·006
	κ =	351	357	24	352	355
O	H =	0·663	0·676	0·682	0·657	0·658
	κ =	48	48	48	48	48
K_1	H =	1·393	1·401	1·398	1·405	1·396
	κ =	45	45	46	45	45
K_2	H =	0·383	0·435	0·415	0·364	0·405
	κ =	355	351	346	352	352
P	H =	0·391	0·416	0·415	0·404	0·404
	κ =	45	44	43	44	43
J	H =	0·109	0·143	0·090	0·048	0·094
	κ =	40	52	86	90	70
Q	H =	0·129	0·147	0·132	0·133	0·133
	κ =	59	49	36	40	49
L	H =	0·082	0·079	0·041	0·095	0·088
	κ =	242	328	305	323	308

II.—Table of Harmonic Constants at Old Indian Ports.

Bombay.

Commence 0 h., January 1.

Year		1883.	1884.	1885.	1886.	Mean of 9 years *
N	H =	0·988	0·978	0·995	1·001	0·997
	κ =	314	312	313	312	313
2N	H =	0·110	0·142	0·153	0·182	0·151
	κ =	291	299	246	278	281
λ	H =	0·044	0·017	0·004	.....	0·028 (8)
	κ =	266	141	95	.....	210
ν	H =	0·276	0·145	0·052	0·210	0·186
	κ =	296	262	13	348	317
μ	H =	0·200	0·183	0·180	0·185	0·197
	κ =	294	308	295	317	306
R	H =	0·046	.....	0·029	.....	0·040 (4)
	κ =	292	.....	227	.....	271
T	H =	0·120	.....	0·237	.....	0·175 (4)
	κ =	52	.....	350	.....	22
MS	H =	0·157	0·137	0·135	0·137	0·135
	κ =	27	22	21	23	24
2SM	H =	0·036	0·049	0·046	0·029	0·038
	κ =	116	113	100	98	106
MN	H =	0·124	0·070	0·130	0·096	0·112
	κ =	266	318	237	292	273
MK	H =	0·084	0·030	0·103	0·008	0·065
	κ =	215	75	131	181	154
2MK	H =	0·070	0·080	0·065	0·062	0·059
	κ =	70	55	51	49	68
Mm	H =	0·063	0·034	0·026	0·045	0·050
	κ =	94	23	64	284	26
Mf	H =	0·046	0·046	0·083	0·061	0·055
	κ =	333	3	49	64	2
MSf	H =	0·044	0·053	0·052	0·036	0·038
	κ =	190	187	268	198	220
Sa	H =	0·032	0·062	0·042	0·110	0·131
	κ =	285	326	99	17	320
Ssa	H =	0·157	0·099	0·042	0·176	0·120
	κ =	186	209	221	148	212

* Except where noted thus (4), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Beypore.

Commence 0 h., December 1.

Year	1883–4.	Mean of 6 years.	Year	1883–4.	Mean of 6 years.*
S_1 H =	0·048	0·059	N H =	0·221	0·201
κ =	172	174	κ =	296	303
S_2 H =	0·350	0·333	2N H =	0·019	0·025
κ =	11	17	κ =	243	251
S_4 H =	0·007	0·005	λ H =	0·002	0·010
κ =	128	135	κ =	253	303
S_6 H =	0·009	0·006	ν H =	0·003	0·046
κ =	245	247	κ =	15	322
S_8 H =	0·003	0·001	μ H =	0·009	0·018
κ =	96	359	κ =	269	260
M_1 H =	0·055	0·033	R H =	0·018	0·019 (3)
κ =	61	71	κ =	126	130
M_2 H =	0·999	0·943	T H =	0·061	0·047 (3)
κ =	324	328	κ =	17	18
M_3 H =	0·008	0·010	MS H =	0·015	0·010
κ =	199	198	κ =	60	74
M_4 H =	0·027	0·021	2SM H =	0·004	0·005
κ =	23	38	κ =	1	306
M_6 H =	0·013	0·008	MN H =	0·016	0·033
κ =	106	133	κ =	38	350
M_8 H =	0·009	0·009	MK H =	0·003	0·014
κ =	158	148	κ =	335	51
O H =	0·362	0·344	2MK H =	0·004	0·010
κ =	56	57	κ =	133	71
K_1 H =	0·730	0·708	Mm H =	0·031	0·081
κ =	48	51	κ =	144	50
K_2 H =	0·105	0·084	Mf H =	0·054	0·068
κ =	0	9	κ =	158	46
P H =	0·230	0·198	MSf H =	0·037	0·038
κ =	51	53	κ =	202	214
J H =	0·078	0·049	Sa H =	0·308	0·309
κ =	34	58	κ =	301	311
Q H =	0·091	0·083	Ssa H =	0·118	0·166
κ =	62	66	κ =	208	205
L H =	0·033	0·027			
κ =	2	350			

* Except where noted thus (3), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Negapatam.

Commence 0 h., March 20.

Year		1885–6.	1886–7.	1887–8.	Mean of 5 years.
S_1	$H =$	0·040	0·021	0·055	0·042
	$\kappa =$	96	97	120	106
S_2	$H =$	0·284	0·261	0·249	0·268
	$\kappa =$	281	281	285	283
S_4	$H =$	0·006	0·006	0·004	0·005
	$\kappa =$	107	126	140	135
S_6	$H =$	0·001	0·001	0·002	0·001
	$\kappa =$	146	252	98	159
S_8	$H =$	0·001	0·001	0·000	0·001
	$\kappa =$	241	219	153	213
M_1	$H =$	0·017	0·016	0·008	0·010
	$\kappa =$	303	289	4	308
M_2	$H =$	0·739	0·706	0·654	0·708
	$\kappa =$	249	251	253	251
M_3	$H =$	0·004	0·002	0·004	0·003
	$\kappa =$	85	73	78	89
M_4	$H =$	0·017	0·021	0·031	0·022
	$\kappa =$	71	76	96	79
M_6	$H =$	0·011	0·010	0·009	0·011
	$\kappa =$	124	135	134	130
M_8	$H =$	0·004	0·003	0·001	0·003
	$\kappa =$	252	335	149	268
O	$H =$	0·087	0·087	0·088	0·089
	$\kappa =$	318	326	321	322
K_1	$H =$	0·224	0·216	0·210	0·220
	$\kappa =$	347	349	349	347
K_2	$H =$	0·078	0·097	0·091	0·084
	$\kappa =$	285	286	282	285
P	$H =$	0·080	0·075	0·074	0·079
	$\kappa =$	340	348	344	345
J	$H =$	0·019	0·014	0·008	0·013
	$\kappa =$	357	35	356	353
Q	$H =$	0·007	0·001	0·008	0·005
	$\kappa =$	284	310	34	270
L	$H =$	0·089	0·047	0·030	0·034
	$\kappa =$	265	219	272	263

II.—Table of Harmonic Constants at Old Indian Ports.

Negapatam.

Commence 0 h., March 20.

Year	1885–6.	1886–7.	1887–8.	Mean of 5 years.*
N { H =	0·168	0·151	0·157	0·158
N { κ =	237	232	239	239
2N { H =	0·035	0·015	0·020	0·025
2N { κ =	219	183	214	210
λ { H =	0·016	0·031	.....	0·019 (4)
λ { κ =	307	324	.....	273
ν { H =	0·039	0·015	0·020	0·034
ν { κ =	209	273	279	239
μ { H =	0·016	0·015	0·014	0·017
μ { κ =	128	103	104	116
R { H =	.....	0·031	.....	0·031 (2)
R { κ =	.....	300	.....	325
T { H =	.....	0·037	.....	0·044 (2)
T { κ =	.....	243	.....	249
MS { H =	0·018	0·018	0·024	0·019
MS { κ =	86	107	111	99
2SM { H =	0·006	0·003	0·006	0·006
2SM { κ =	198	230	208	203
MN { H =	0·024	0·048	0·022	0·028
MN { κ =	121	182	155	123
MK { H =	0·010	0·015	0·020	0·014
MK { κ =	69	144	195	149
2MK { H =	0·006	0·009	0·007	0·007
2MK { κ =	335	336	336	337
Mm { H =	0·076	0·008	0·048	0·049
Mm { κ =	318	347	352	335
Mf { H =	0·080	0·098	0·073	0·066
Mf { κ =	354	5	351	1
MSf { H =	0·025	0·026	0·043	0·055
MSf { κ =	82	51	15	33
Sa { H =	0·348	0·444	0·364	0·444
Sa { κ =	249	230	228	234
Ssa { H =	0·300	0·328	0·377	0·344
Ssa { κ =	129	129	121	128

* Except where noted thus (2), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Madras.

Commence 0 h., February 1.

Year	1883–4.	1884–5.	1885–6.	Mean of 6 years.
S_1 { H =	0·026	0·056	0·017	0·029
S_1 { κ =	88	100	75	90
S_2 { H =	0·436	0·450	0·415	0·437
S_2 { κ =	280	280	290	280
S_4 { H =	0·002	0·005	0·003	0·003
S_4 { κ =	217	302	288	215
S_6 { H =	0·001	0·001	0·001	0·001
S_6 { κ =	56	63	66	87
S_8 { H =	0·000	0·001	0·001	0·001
S_8 { κ =	198	333	50	298
M_1 { H =	0·003	0·038	0·018	0·014
M_1 { κ =	41	283	269	342
M_2 { H =	1·033	1·058	0·983	1·037
M_2 { κ =	250	248	259	250
M_3 { H =	0·004	0·003	0·003	0·004
M_3 { κ =	57	8	0	42
M_4 { H =	0·002	0·019	0·014	0·007
M_4 { κ =	154	226	225	174
M_6 { H =	0·006	0·008	0·006	0·008
M_6 { κ =	160	165	204	165
M_8 { H =	0·002	0·001	0·003	0·002
M_8 { κ =	29	19	192	63
O { H =	0·096	0·100	0·089	0·096
O { κ =	331	322	333	327
K_1 { H =	0·291	0·296	0·286	0·292
K_1 { κ =	342	341	346	341
K_2 { H =	0·116	0·086	0·118	0·109
K_2 { κ =	268	269	305	280
P { H =	0·091	0·104	0·090	0·096
P { κ =	344	346	348	345
J { H =	0·022	0·030	0·006	0·020
J { κ =	318	346	323	324
Q { H =	0·002	0·007	0·009	0·006
Q { κ =	68	280	96	130
L { H =	0·037	0·026	0·040	0·035
L { κ =	287	359	299	311

II.—Table of Harmonic Constants at Old Indian Ports.

Madras.

Commence 0 h., February 1.

Year		1883-4.	1884-5.	1885-6.	Mean of 6 years.*
N	H =	0·229	0·265	0·193	0·234
	κ =	244	238	250	243
2N	H =	0·044	0·061	0·032	0·042
	κ =	229	201	288	242
λ	H =	0·009	0·071	0·012	0·030
	κ =	216	73	222	295
ν	H =	0·079	0·145	0·050	0·068
	κ =	255	224	177	245
μ	H =	0·046	0·063	0·063	0·049
	κ =	190	195	170	182
R	H =	0·016	.....	0·053	0·028 (3)
	κ =	358	.....	146	202
T	H =	0·019	.....	0·080	0·052 (3)
	κ =	19	.....	225	167
MS	H =	0·002	0·015	0·010	0·006
	κ =	37	257	270	179
2SM	H =	0·018	0·021	0·009	0·019
	κ =	233	257	236	225
MN	H =	0·040	0·102	0·021	0·044
	κ =	140	77	101	114
MK	H =	0·014	0·025	0·010	0·014
	κ =	291	10	85	57
2MK	H =	0·005	0·006	0·007	0·007
	κ =	52	14	103	64
Mm	H =	0·027	0·017	0·056	0·040
	κ =	285	0	336	83
Mf	H =	0·044	0·020	0·054	0·042
	κ =	65	25	343	15
MSf	H =	0·023	0·026	0·035	0·023
	κ =	30	128	334	51
Sa	H =	0·520	0·366	0·351	0·399
	κ =	235	215	228	219
Ssa	H =	0·300	0·362	0·289	0·311
	κ =	139	137	140	133

* Except where noted thus (3), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

	Vizagapatam. Commence 0 h., February 3.			*False Point.* Commence 0 h, May 1.		
Year	1883–4.	1884–5.	Mean of 6 years.	1883–4.	1884–5.	Mean of 4 years.
S_1 { H =	0·037	0·044	0·048	0·006	0·008	0·011
κ =	93	94	76	48	86	37
S_2 { H =	0·640	0·625	0·648	0·993	1·000	1·007
κ =	287	288	286	302	298	302
S_4 { H =	0·004	0·003	0·005	0·009	0·006	0·008
κ =	67	45	50	316	307	320
S_6 { H =	0·001	0·001	0·001	0·003	0·005	0·004
κ =	146	114	157	163	158	165
S_8 { H =	0·001	0·000	0·001	0·004	0·005	0·004
κ =	76	288	53	281	181	235
M_1 { H =	0·007	0·016	0·012	0·014	0·009	0·010
κ =	351	289	303	287	227	324
M_2 { H =	1·464	1·462	1·469	2·267	2·237	2·251
κ =	255	256	254	269	267	269
M_3 { H =	0·007	0·009	0·006	0·012	0·016	0·014
κ =	10	22	345	36	27	31
M_4 { H =	0·013	0·004	0·013	0·035	0·029	0·035
κ =	11	227	320	224	233	229
M_6 { H =	0·004	0·007	0·005	0·014	0·004	0·010
κ =	61	66	69	44	142	78
M_8 { H =	0·005	0·004	0·004	0·006	0·004	0·004
κ =	215	241	215	192	220	226
O { H =	0·138	0·129	0·139	0·176	0·172	0·176
κ =	332	333	332	334	334	335
K_1 { H =	0·355	0·358	0·358	0·413	0·406	0·409
κ =	342	343	342	344	341	344
K_2 { H =	0·181	0·163	0·192	0·289	0·292	0·273
κ =	279	279	278	307	295	299
P { H =	0·116	0·109	0·101	0·127	0·132	0·137
κ =	340	345	341	346	344	345
J { H =	0·026	0·024	0·025	0·031	0·020	0·026
κ =	343	18	345	329	359	328
Q { H =	0·020	0·014	0·012	0·013	0·005	0·010
κ =	348	338	331	312	187	287
L { H =	0·046	0·078	0·055	0·068	0·095	0·070
κ =	281	256	259	266	286	265

II.—Table of Harmonic Constants at Old Indian Ports.

Vizagapatam. Commence 0 h., February 3. — *False Point.* Commence 0 h., May 1.

Year ……		Vizagapatam 1883–4.	Vizagapatam 1884–5.	Vizagapatam Mean of 6 years.*	False Point 1883–4.	False Point 1884–5.	False Point Mean of 4 years.*
N	H =	0·296	0·298	0·308	0·425	0·439	0·454
	κ =	248	252	248	264	258	264
2N	H =	0·039	0·056	0·052	0·066	0·050	0·068
	κ =	244	218	233	238	240	249
λ	H =	0·012	0·039	0·023	0·019	0·066	0·053
	κ =	214	299	261	331	272	331
ν	H =	0·116	0·095	0·085	0·036	0·136	0·114
	κ =	257	223	213	305	301	273
μ	H =	0·028	0·036	0·028	0·069	0·042	0·065
	κ =	258	264	260	265	252	266
R	H =	……	0·025	0·026 (3)	……	0·014	0·024 (2)
	κ =	……	69	148	……	284	250
T	H =	……	0·036	0·046 (3)	……	0·099	0·058 (2)
	κ =	……	282	269	……	280	215
MS	H =	0·012	0·007	0·011	0·041	0·039	0·040
	κ =	28	283	356	266	261	269
2SM	H =	0·004	0·012	0·011	0·020	0·028	0·020
	κ =	312	220	239	189	213	194
MN	H =	0·042	0·030	0·037	0·017	0·047	0·051
	κ =	30	59	37	0	27	21
MK	H =	0·022	0·022	0·018	0·027	0·015	0·026
	κ =	334	25	358	101	227	258
2MK	H =	0·010	0·015	0·012	0·010	0·010	0·010
	κ =	323	327	329	346	1	340
Mm	H =	0·029	0·010	0·043	0·045	0·014	0·046
	κ =	265	7	21	115	43	67
Mf	H =	0·082	0·073	0·054	0·067	0·099	0·075
	κ =	47	32	14	13	32	29
MSf	H =	0·025	0·019	0·038	0·039	0·014	0·038
	κ =	358	39	22	158	242	278
Sa	H =	0·612	0·694	0·694	0·841	0·888	0·829
	κ =	195	182	184	172	162	166
Ssa	H =	0·364	0·350	0·340	0·282	0·260	0·279
	κ =	127	129	119	154	158	151

* Except where noted thus (3), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Dublat.

Commence 0 h., April 22.

Year		1883–4.	1884–5.	1885–6.	Mean of 5 years.
S_1	H =	0·040	0·047	0·047	0·046
	κ =	142	124	131	124
S_2	H =	2·147	2·071	2·099	2·107
	κ =	329	326	330	328
S_4	H =	0·017	0·015	0·011	0·016
	κ =	201	255	237	223
S_6	H =	0·005	0·001	0·002	0·003
	κ =	40	59	259	111
S_8	H =	0·003	0·002	0·009	0·005
	κ =	88	58	130	101
M_1	H =	0·017	0·024	0·027	0·017
	κ =	62	265	291	356
M_2	H =	4·594	4·626	4·603	4·608
	κ =	290	290	294	291
M_3	H =	0·051	0·048	0·049	0·048
	κ =	138	133	137	135
M_4	H =	0·081	0·086	0·081	0·088
	κ =	149	149	160	149
M_6	H =	0·008	0·013	0·007	0·011
	κ =	250	165	181	221
M_8	H =	0·012	0·006	0·009	0·010
	κ =	279	302	298	294
O	H =	0·186	0·183	0·196	0·189
	κ =	342	343	336	338
K_1	H =	0·503	0·490	0·493	0·494
	κ =	352	350	354	352
K_2	H =	0·599	0·634	0·691	0·623
	κ =	328	333	327	325
P	H =	0·141	0·166	0·148	0·151
	κ =	347	350	350	347
J	H =	0·022	0·058	0·033	0·031
	κ =	307	2	17	339
Q	H =	0·013	0·012	0·010	0·011
	κ =	11	312	58	353
L	H =	0·210	0·170	0·245	0·198
	κ =	295	300	302	296

II.—Table of Harmonic Constants at Old Indian Ports.

Dublat.

Commence 0 h., April 22.

Year	1883-4.	1884-5.	1885-6.	Mean of 5 years.*
N { H =	0·820	0·875	0·882	0·894
N { κ =	285	283	287	285
2N { H =	0·096	0·200	0·147	0·155
2N { κ =	221	253	264	261
λ { H =	0·085	0·063	0·163	0·150
λ { κ =	261	277	325	299
ν { H =	0·142	0·276	0·328	0·242
ν { κ =	295	303	276	275
μ { H =	0·172	0·107	0·141	0·150
μ { κ =	14	355	10	10
R { H =	....	0·095	....	0·157 (2)
R { κ =	....	307	....	298 (2)
T { H =	....	0·175	....	0·156 (2)
T { κ =	....	61	....	0 (2)
MS { H =	0·067	0·074	0·077	0·074
MS { κ =	174	177	191	170
2SM { H =	0·053	0·058	0·044	0·060
2SM { κ =	193	198	196	202
MN { H =	0·172	0·050	0·198	0·120
MN { κ =	55	70	20	355
MK { H =	0·023	0·053	0·072	0·062
MK { κ =	353	142	192	225
2MK { H =	0·028	0·050	0·031	0·035
2MK { κ =	125	124	97	129
Mm { H =	0·060	0·027	0·020	0·037
Mm { κ =	75	43	171	89
Mf { H =	0·092	0·086	0·032	0·061
Mf { κ =	46	34	86	60
MSf { H =	0·050	0·027	0·042	0·049
MSf { κ =	128	234	26	292
Sa { H =	0·864	0·930	0·787	0·878
Sa { κ =	153	146	154	151
Ssa { H =	0·202	0·211	0·146	0·195
Ssa { κ =	134	162	137	141

* Except where noted thus (2), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Diamond Harbour.

Commence 0 h., April 4.

Year		1883–4.	1884–5.	1885–6.	Mean of 5 years.
S_1	H =	0·093	0·092	0·101	0·091
	κ =	150	161	163	155
S_2	H =	2·252	2·202	2·199	2·231
	κ =	26	26	26	26
S_4	H =	0·132	0·123	0·123	0·123
	κ =	330	329	326	327
S_6	H =	0·015	0·013	0·006	0·012
	κ =	268	270	233	254
S_8	H =	0·004	0·007	0·002	0·004
	κ =	241	286	175	282
M_1	H =	0·022	0·052	0·032	0·029
	κ =	145	203	277	163
M_2	H =	5·177	5·135	5·154	5·164
	κ =	344	345	345	344
M_3	H =	0·061	0·062	0·058	0·050
	κ =	245	237	225	230
M_4	H =	0·752	0·753	0·765	0·752
	κ =	246	249	250	247
M_6	H =	0·163	0·141	0·144	0·150
	κ =	106	112	110	108
M_8	H =	0·060	0·053	0·053	0·058
	κ =	344	349	354	347
O	H =	0·211	0·217	0·233	0·226
	κ =	342	350	348	346
K_1	H =	0·508	0·498	0·515	0·502
	κ =	16	14	13	14
K_2	H =	0·730	0·718	0·622	0·676
	κ =	25	23	30	25
P	H =	0·178	0·184	0·171	0·176
	κ =	9	12	11	10
J	H =	0·006	0·035	0·045	0·030
	κ =	68	28	24	8
Q	H =	0·036	0·019	0·016	0·026
	κ =	304	301	44	350
L	H =	0·201	0·280	0·276	0·256
	κ =	335	344	8	350

II.—Table of Harmonic Constants at Old Indian Ports.

Diamond Harbour.

Commence 0 h., April 4.

Year		1883–4.	1884–5.	1885–6.	Mean of 5 years.
N	H =	0·898	0·945	1·030	0·955
	κ =	336	336	347	340
2N	H =	0·212	0·167	0·147	0·148
	κ =	288	314	321	334
λ	H =	0·046	0·192	0·267	0·147
	κ =	22	357	358	354
ν	H =	0·204	0·387	0·203	0·280
	κ =	346	331	299	311
μ	H =	0·298	0·338	0·268	0·302
	κ =	90	82	85	85
R	H =	....	0·175	....	0·196(2)
	κ =	....	17	....	13
T	H =	....	0·317	....	0·198(2)
	κ =	....	86	....	71
MS	H =	0·702	0·728	0·709	0·706
	κ =	288	289	288	287
2SM	H =	0·058	0·069	0·074	0·070
	κ =	274	271	290	275
MN	H =	0·100	0·085	0·116	0·118
	κ =	71	25	68	52
MK	H =	0·124	0·159	0·107	0·117
	κ =	249	279	301	281
2MK	H =	0·066	0·059	0·065	0·061
	κ =	214	220	201	217
Mm	H =	0·156	0·145	0·078	0·117
	κ =	26	17	3	10
Mf	H =	0·216	0·155	0·096	0·158
	κ =	57	40	33	42
MSf	H =	0·453	0·424	0·488	0·452
	κ =	41	36	29	34
Sa	H =	0·980	0·991	1·119	1·058
	κ =	141	143	140	142
Ssa	H =	0·103	0·069	0·182	0·097
	κ =	92	150	262	129

* Except where noted thus (2), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Kidderpore.

Commence 0 h., March 22.

Year		1883–4.	1884–5.	1885–6.	1886–7.	Mean of 6 years.
S_1	N =	0·097	0·082	0·088	0·082	0·089
	κ =	193	200	205	197	197
S_2	H =	1·513	1·462	1·459	1·482	1·475
	κ =	103	104	102	98	102
S_4	H =	0·095	0·080	0 074	0·093	0·082
	κ =	124	118	117	108	117
S_6	H =	0·003	0·001	0·008	0·005	0 005
	κ =	59	194	340	41	325
S_8	H =	0·002	0·007	0·005	0·008	0 005
	κ =	227	235	285	297	278
M_1	H =	0·034	0·052	0·051	0·039	0·034
	κ =	178	260	335	355	240
M_2	H =	3·646	3·674	3·627	3·521	3·620
	κ =	58	60	60	58	59
M_3	H =	0·028	0·043	0·060	0·056	0·036
	κ =	350	344	333	315	334
M_4	H =	0·691	0·729	0·736	0·714	0·720
	κ =	36	40	42	40	39
M_6	H =	0·156	0·156	0·161	0·144	0·156
	κ =	310	325	331	324	321
M_8	H =	0·073	0·067	0·065	0·070	0·072
	κ =	268	273	284	277	274
O	H =	0·206	0·210	0·209	0·194	0·210
	κ =	16	23	23	23	21
K_1	H =	0·400	0·398	0·394	0·384	0·392
	κ =	55	55	57	54	55
K_2	H =	0·504	0·489	0·381	0·451	0·449
	κ =	103	98	95	96	97
P	H =	0·140	0·153	0·132	0·136	0·142
	κ =	49	51	40	40	46
J	H =	0·017	0·031	0·011	0·004	0·015
	κ =	317	50	82	274	349
Q	H =	0·036	0·034	0·016	0·011	0·029
	κ =	350	350	14	349	0
L	H =	0·222	0·151	0·221	0·210	0·196
	κ =	59	63	74	65	68

II.—Table of Harmonic Constants at Old Indian Ports.

Kidderpore.

Commence 0 h., March 22.

Year		1883–4.	1884–5.	1885–6.	1886–7.	Mean of 6 years.*
N	H =	0 628	0·662	0·675	0·649	0·648
	κ =	42	45	47	45	46
2N	H =	0·124	0·127	0·099	0·059	0·088
	κ =	355	34	8	37	34
λ	H =	0 091	0·055	0·098	.....	0·089 (5)
	κ =	44	73	134	.....	93
ν	H =	0·170	0·318	0·320	0·185	0·245
	κ =	62	44	13	3	18
μ	H =	0·294	0·220	0·206	0·203	0·235
	κ =	181	183	191	203	187
R	H =	.....	0·123	.....	.....	0·145 (2)
	κ =	.....	79	.....	.....	78
T	H =	.....	0·175	.....	0·127	0·150 (3)
	κ =	.....	184	.....	87	126
MS	H =	0 645	0·625	0·654	0·651	0·644
	κ =	82	85	85	82	83
2SM	H =	0·063	0·066	0·096	0·089	0·081
	κ =	15	13	17	17	11
MN	H =	0·108	0·105	0·043	0·146	0·108
	κ =	293	228	131	235	227
MK	H =	0·144	0·085	0·082	0·123	0·108
	κ =	39	61	26	21	31
2MK	H =	0·032	0·032	0·040	0·028	0·034
	κ =	296	324	301	262	311
Mm	H =	0·290	0·288	0·2[illegible]	0·287	0·270
	κ =	22	12	18	353	4
Mf	H =	0·346	0·238	0·317	0·263	0·293
	κ =	54	54	34	19	40
MSf	H =	0·905	0·834	0·981	0·979	0·906
	κ =	47	43	40	41	41
Sa	H =	2·312	2·361	3·006	3·114	2·712
	κ =	150	162	161	163	158
Ssa	H =	0·714	0·651	1·307	1·092	0·901
	κ =	322	353	328	345	314

* Except where noted thus (5), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Rangoon.

Commence 0 h., March 1.

Year		1883–4.	1884–5.	1885–6.	Mean of 6 years.
S_1	H =	0·118	0·105	0·106	0 112
	κ =	130	129	139	133
S_2	H =	1·995	2·021	1·922	1·996
	κ =	170	172	172	171
S_4	H =	0·083	0·088	0·083	0·083
	κ =	257	265	261	260
S_6	H =	0·007	0·011	0·011	0·010
	κ =	58	32	48	47
S_8	H =	0·002	0·007	0·005	0·005
	κ =	115	97	133	117
M_1	H =	0·029	0·031	0·017	0·029
	κ =	126	52	144	145
M_2	H =	5·588	5·635	5·609	5·578
	κ =	131	132	133	132
M_3	H =	0·024	0·031	0·030	0·025
	κ =	151	70	15	128
M_4	H =	0·441	0·419	0·405	0·416
	κ =	169	171	175	170
M_6	H =	0·228	0·226	0·228	0·230
	κ =	87	89	92	88
M_8	H =	0·094	0·089	0·091	0·086
	κ =	95	99	109	99
O	H =	0·297	0·287	0·283	0 292
	κ =	33	31	32	30
K_1	H =	0·666	0·668	0·669	0·669
	κ =	37	38	37	36
K_2	H =	0·543	0·578	0·699	0·588
	κ =	163	173	190	172
P	H =	0·184	0·167	0·139	0·148
	κ =	49	55	57	55
J	H =	0·034	0·039	0·033	0·033
	κ =	38	90	135	60
Q	H =	0·045	0·036	0·021	0·030
	κ =	68	39	40	40
L	H =	0·426	0·444	0·283	0·396
	κ =	143	150	131	149

II.—Table of Harmonic Constants at Old Indian Ports.

Rangoon.

Commence 0 h., March 1.

Year		1883–4.	1884–5	1885-6.	Mean of 6 years.*
N	H =	1·006	1·050	1·074	1·017
	κ =	115	116	118	117
2N	H =	0·108	0·233	0·118	0·149
	κ =	82	74	125	97
λ	H =	0·203	0·320	0·228	0·254
	κ =	143	169	197	170
ν	H =	0·383	0·508	0·455	0·383
	κ =	138	109	98	107
μ	H =	0·478	0·506	0·566	0·515
	κ =	288	288	292	290
R	H =	0·096	.....	0·112	0·108 (3)
	κ =	125	.....	45	79
T	H =	0·222	.....	0·289	0·267 (3)
	κ =	183	.....	124	145
MS	H =	0·421	0·386	0·393	0·398
	κ =	213	214	218	212
2SM	H =	0·175	0·154	0·187	0·166
	κ =	61	50	56	54
MN	H =	0·154	0·096	0·275	0·168
	κ =	36	31	11	26
MK	H =	0·118	0·090	0·166	0·140
	κ =	102	63	66	73
2MK	H =	0·124	0·116	0·121	0·119
	κ =	56	61	49	55
Mm	H =	0·279	0·171	0·206	0·227
	κ =	15	5	12	17
Mf	H =	0·228	0·270	0·171	0·216
	κ =	46	29	37	36
MSf	H =	0·541	0·580	0·542	0·546
	κ =	46	51	51	49
Sa	H =	1·405	1·201	1·184	1·375
	κ =	157	146	150	151
Ssa	H =	0·174	0·071	0·228	0·142
	κ =	1	263	298	318

* Except where noted thus (3), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Amherst.

Commence 0 h., August 5.

Year		1883–4.	1884–5.	1885–6.	Mean of 6 years.
S_1	H =	0·124	0·137	0·131	0·176
	κ =	120	133	122	133
S_2	H =	2·680	2·700	2·563	2·708
	κ =	100	95	102	102
S_4	H =	0·080	0·099	0·075	0·095
	κ =	108	101	108	114
S_6	H =	0·008	0·002	0·002	0·008
	κ =	328	164	342	233
S_8	H =	0·003	0·002	0·003	0·005
	κ =	302	267	244	273
M_1	H =	0·014	0·038	0·045	0·032
	κ =	88	93	29	343
M_2	H =	6·376	6·427	6·415	6·320
	κ =	66	65	67	67
M_3	H =	0·021	0·033	0·031	0·024
	κ =	275	237	260	259
M_4	H =	0·303	0·315	0·273	0·324
	κ =	37	36	32	43
M_6	H =	0·138	0·142	0·151	0·131
	κ =	254	250	249	252
M_8	H =	0·016	0·021	0·023	0·017
	κ =	219	222	240	238
O	H =	0·339	0·335	0·310	0·323
	κ =	345	347	349	343
K_1	H =	0·714	0·702	0·738	0·709
	κ =	3	1	4	4
K_2	H =	0·883	0·973	0·752	0·987
	κ =	101	96	111	96
P	H =	0·207	0·195	0·212	0·191
	κ =	3	6	12	352
J	H =	0·022	0·028	0·045	0·053
	κ =	11	59	73	41
Q	H =	0·018	0·020	0·035	0·039
	κ =	11	7	347	342
L	H =	0·362	0·373	0·314	0·321
	κ =	81	90	78	97

II.—Table of Harmonic Constants at Old Indian Ports.

Amherst.

Commence 0 h., August 5.

Year		1883–4.	1884–5.	1885–6.	Mean of 6 years.*
N	H =	1·230	1·194	1·312	1·284
	κ =	52	51	48	52
2N	H =	0·271	0·204	0·173	0·245
	κ =	23	72	61	34
λ	H =	0·185	0·178	0·216	0·246
	κ =	92	133	184	127
ν	H =	0·428	0·232	0·099	0·339
	κ =	49	25	55	50
μ	H =	0·274	0·202	0·326	0·285
	κ =	310	281	293	298
R	H =	0·033	.....	0·174	0·219 (3)
	κ =	347	.....	316	305
T	H =	0·074	.....	0·352	0·422 (3)
	κ =	284	.....	79	169
MS	H =	0·291	0·300	0·275	0·318
	κ =	73	66	64	75
2SM	H =	0·176	0·181	0·176	0·164
	κ =	5	13	328	3
MN	H =	0·271	0·198	0·035	0·214
	κ =	216	244	159	210
MK	H =	0·011	0·102	0·122	0·091
	κ =	280	302	348	335
2MK	H =	0·039	0·044	0·037	0·051
	κ =	309	320	313	315
Mm	H =	0·109	0·049	0·006	0·071 (5)
	κ =	342	4	290	2
Mf	H =	0·083	0·107	0·017	0·080 (5)
	κ =	328	34	213	327
Msf	H =	0·052	0·067	0·068	0·059 (5)
	κ =	134	69	306	58
Sa	H =	0·739	0·713	0·886	0·758 (5)
	κ =	149	147	107	136
Ssa	H =	0·161	0·119	0·154	0·149 (5)
	κ =	107	181	154	111

* Except where noted thus (3), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Moulmein.

Commence 0 h., April 17.

Year	1883–4.	1884–5.	1885–6.	Mean of 6 years.
S_1 {H =	0·099	0·114	0·074	0 096
S_1 {κ =	151	144	154	149
S_2 {H =	1·349	1·364	1·364	1·361
S_2 {κ =	149	150	151	149
S_4 {H =	0·062	0·071	0·073	0·068
S_4 {κ =	228	223	228	228
S_6 {H =	0·005	0 007	0·007	0·006
S_6 {κ =	261	246	222	213
S_8 {H =	0·002	0·002	0·000	0·002
S_8 {κ =	320	121	198	212
M_1 {H =	0·029	0·019	0·026	0·022
M_1 {κ =	145	122	71	125
M_2 {H =	3·720	3·887	3·803	3·791
M_2 {κ =	113	114	115	114
M_3 {H =	0·020	0·019	0·028	0·024
M_3 {κ =	165	117	42	159
M_4 {H =	0·869	0·906	0·897	0·896
M_4 {κ =	171	173	176	172
M_6 {H =	0·093	0·077	0·084	0·094
M_6 {κ =	197	208	218	204
M_8 {H =	0·040	0·043	0·036	0·039
M_8 {κ =	136	119	123	130
O {H =	0·275	0·273	0·246	0·259
O {κ =	51	55	54	51
K_1 {H =	0·425	0·456	0·429	0·437
K_1 {κ =	41	44	43	42
K_2 {H =	0·371	0·275	0·309	0·327
K_2 {κ =	164	158	159	158
P {H =	0·119	0·145	0·116	0·130
P {κ =	54	53	54	57
J {H =	0·022	0·016	0·015	0·020
J {κ =	22	63	72	80
Q {H =	0·042	0·056	0·046	0·047
Q {κ =	57	79	57	59
L {H =	0·320	0·330	0·297	0·297
L {κ =	136	123	144	137

II.—Table of Harmonic Constants at Old Indian Ports.

Moulmein.

Commence 0 h., April 17.

Year		1883–4.	1884–5.	1885–6.	Mean of 6 years.
N	H =	0·654	0·620	0·713	0·671
	κ =	95	92	99	99
2N	H =	0·120	0·082	0·120	0·098
	κ =	79	145	74	86
λ	H =	0·104	0·183	0·165	0·163
	κ =	107	153	170	154
ν	H =	0·173	0·435	0·331	0·273
	κ =	126	128	84	98
μ	H =	0·347	0·320	0·339	0·324
	κ =	274	260	279	271
R	H =	0·133	....	0·204	0·146 (3)
	κ =	79	....	72	73
T	H =	0·151	....	0·264	0·205 (3)
	κ =	174	....	100	128
MS	H =	0·685	0·714	0·715	0·708
	κ =	213	215	218	213
2SM	H =	0·123	0·155	0·118	0·128
	κ =	39	50	40	41
MN	H =	0·126	0·203	0·086	0·135
	κ =	30	36	4	19
MK	H =	0·197	0·162	0·133	0·164
	κ =	93	103	87	89
2MK	H =	0·111	0·099	0·111	0·112
	κ =	70	57	61	62
Mm	H =	0·407	0·344	0·369	0·367
	κ =	19	5	9	12
Mf	H =	0·377	0·217	0·371	0·328
	κ =	49	32	32	39
MSf	H =	1·091	1·050	1·063	1·089
	κ =	45	42	45	45
Sa	H =	2·519	2·032	2·128	2·330
	κ =	152	144	151	149
Ssa	H =	0·653	0·501	0·730	*0·616
	κ =	298	268	288	286

* Except where noted thus (3), where this represents the number of years.

II.—Table of Harmonic Constants at Old Indian Ports.

Port Blair.

Commence 0 h., April 19.

Year	1883–4.	1884–5.	1885–6.	1886–7.	Mean of 7 years.
S_1 H =	0·015	0·051	0·006	0·024	0·023
κ =	85	28	125	79	62
S_2 H =	0·975	0·963	0·983	0·953	0·961
κ =	316	320	322	317	317
S_4 H =	0·004	0·004	0·004	0·002	0·003
κ =	108	126	68	257	64
S_6 H =	0·003	0·001	0·002	0·003	0·002
κ =	176	167	99	118	136
S_8 H =	0·001	0·000	0·002	0·002	0·001
κ =	221	278	114	50	129
M H =	0·004	0·028	0·032	0·017	0·016
κ =	313	288	315	322	302
M_2 H =	2·013	2·029	1·951	1·986	2·006
κ =	279	282	285	281	280
M_3 H =	0·009	0·005	0·004	0·007	0·007
κ =	25	28	41	14	22
M_4 H =	0·013	0·017	0·016	0·008	0·011
κ =	99	112	108	76	121
M_6 H =	0·007	0·002	0·008	0·006	0·004
κ =	166	133	233	190	239
M_8 H =	0·001	0·001	0·002	0·002	0·002
κ =	80	64	56	95	72
O H =	0·159	0·155	0·162	0·152	0·158
κ =	302	300	304	302	302
K_1 H =	0·398	0·417	0·397	0·397	0·399
κ =	328	330	332	328	328
K_2 H =	0·277	0·179	0·233	0·234	0·253
κ =	315	279	322	311	308
P H =	0·132	0·176	0·129	0·131	0·138
κ =	324	319	327	326	325
J H =	0·021	0·033	0·032	0·015	0·026
κ =	297	305	348	330	322
Q H =	0·011	0·022	0·020	0·014	0·020
κ =	256	255	250	214	241
L H =	0·093	0·049	0·087	0·083	0·074
κ =	288	327	291	269	284

II.—Table of Harmonic Constants at Old Indian Ports.

Port Blair.

Commence 0 h., April 19.

Year		1883–4.	1884–5.	1885–6.	1886–7.	Mean of 7 years.*
N	H =	0·382	0·423	0·391	0·405	0·400
	κ =	272	274	277	273	274
2N	H =	0·044	0·094	0·066	0·070	0·066 (6)
	κ =	241	282	240	282	267
λ	H =	0·036	0·087	.....	.....	0·050 (5)
	κ =	216	176	.....	.....	247
ν	H =	0·020	0·179	0·139	0·100	0·115
	κ =	332	298	281	233	272
μ	H =	0·074	0·121	0·071	0·080	0·086
	κ =	315	280	312	285	296
R	H =	0·022	.....	.....	.....	0·021 (2)
	κ =	261	.....	.....	.....	293
T	H =	0·037	.....	0·112	.....	0·088 (3)
	κ =	355	.....	291	.....	319
MS	H =	0·004	0·007	0·006	0·003	0·007
	κ =	183	107	173	345	208
2SM	H =	0·017	0·022	0·021	0·030	0·023
	κ =	140	330	182	146	180
MN	H =	0·037	0·105	0·024	0·078	0·063 (6)
	κ =	166	97	124	138	131
MK	H =	0·025	0·026	0·025	0·021	0·021 (6)
	κ =	325	57	154	235	195
2MK	H =	0·003	0·004	0·005	0·005	0·005 (6)
	κ =	229	166	260	264	226
Mm	H =	0·010	0·001	0·034	0·023	0·016
	κ =	35	129	341	10	31
Mf	H =	0·053	0·036	0·048	0·025	0·048
	κ =	13	32	32	294	6
Msf	H =	0·014	0·018	0·036	0·027	0·020
	κ =	33	18	354	74	43
Sa	H =	0·218	0·165	0·255	0·048	0·185
	κ =	180	162	147	125	152
Ssa	H =	0·163	0·157	0·201	0·105	0·188
	κ =	177	176	181	237	186

* Except where noted thus (6), where this represents the number of years.

III.—Table of Harmonic Constants at New Indian Ports.

Bhavnagar.

Commence at 0 h., January 1.

Year		1886.	1887.	Mean of 2 years.	Year		1886.	1887.	Mean of 2 years.
S_1	H =	0·154	0·129	0·142	N	H =	2·280	2·521	2·401
	κ =	180	186	183		κ =	111	113	112
S_2	H =	3·376	3·414	3·395	2N	H =	0·271	0·130	0·201
	κ =	176	176	176		κ =	104	27	66
S_4	H =	0·102	0·126	0·114	λ	H =	0·278	....	0·278
	κ =	237	230	234		κ =	142	....	142
S_6	H =	0·027	0·025	0·026	ν	H =	0·640	0·930	0·785
	κ =	308	297	302		κ =	135	108	121
S_8	H =	0·009	0·007	0·008	μ	H =	0·353	0·260	0·307
	κ =	25	94	60		κ =	274	287	281
M_1	H =	0·066	0·126	0·096	R	H =	....	....	....
	κ =	201	157	179		κ =	....	....	....
M_2	H =	10·534	10·724	10·629	T	H =	....	0·277	0·277
	κ =	135	135	135		κ =	....	247	247
M_3	H =	0·078	0·113	0·096	MS	H =	0·638	0·683	0·661
	κ =	317	328	323		κ =	195	197	196
M_4	H =	0·896	0·916	0·906	2SM	H =	0·044	0·057	0·050
	κ =	156	153	154		κ =	12	353	2
M_6	H =	0·228	0·219	0·224	MN	H =	0·210	0·425	0·318
	κ =	119	125	122		κ =	93	93	93
M_8	H =	0·015	0·021	0·018	MK	H =	0·189	0·326	0·258
	κ =	179	130	155		κ =	80	106	93
O	H =	1·011	0·989	1·000	2MK	H =	0·123	0·125	0·124
	κ =	83	84	83		κ =	350	350	350
K_1	H =	2·257	2·323	2·290	Mm	H =	0·107	0·133	0·120
	κ =	92	91	91		κ =	6	39	23
K_2	H =	0·715	0·859	0·787	Mf	H =	0·075	0·053	0·064
	κ =	169	176	173		κ =	39	44	42
P	H =	0·655	0·680	0·668	MSf	H =	0·115	0·220	0·168
	κ =	93	94	94		κ =	28	40	34
J	H =	0·119	0·096	0·107	Sa	H =	0·266	0·376	0·321
	κ =	179	138	158		κ =	121	115	118
Q	H =	0·178	0·207	0·193	Ssa	H =	0·083	0·271	0·177
	κ =	73	88	80		κ =	165	169	167
L	H =	0·589	0·735	0·662					
	κ =	166	150	158					

III.—Table of Harmonic Constants at New Indian Ports.

Mormugão.

Commence 0 h., March 16.

Year	1884–5.	1885–6.	1886–7.	Mean of 3 years.
S_1 H =	0·080	0·041	0·047	0·056
S_1 κ =	157	177	172	169
S_2 H =	0·638	0·641	0·643	0·641
S_2 κ =	337	332	331	333
S_4 H =	0·008	0·009	0·008	0·008
S_4 κ =	109	100	89	99
S_6 H =	0·003	0·005	0·004	0·004
S_6 κ =	120	110	127	119
S_8 H =	0·003	0·004	0·003	0·003
S_8 κ =	95	24	31	50
M_1 H =	0·045	0·055	0·015	0·038
M_1 κ =	98	98	43	80
M_2 H =	1·766	1·820	1·835	1·807
M_2 κ =	305	300	299	302
M_3 H =	0·018	0·015	0·017	0·017
M_3 κ =	308	299	296	301
M_4 H =	0·041	0·047	0·051	0·046
M_4 κ =	21	6	6	11
M_6 H =	0·010	0·013	0·012	0·012
M_6 κ =	261	245	254	253
M_8 H =	0·012	0·011	0·017	0·013
M_8 κ =	24	20	16	20
O H =	0·516	0·524	0·520	0·520
O κ =	53	50	48	50
K_1 H =	1·020	1·033	1·026	1·026
K_1 κ =	48	46	45	46
K_2 H =	0·182	0·179	0·205	0·189
K_2 κ =	324	331	324	327
P H =	0·300	0·305	0·289	0·298
P κ =	49	43	42	45
J H =	0·061	0·085	0·075	0·074
J κ =	43	43	71	52
Q H =	0·099	0·119	0·111	0·110
Q κ =	64	52	42	52
L H =	0·030	0·053	0·039	0·041
L κ =	307	338	303	316

III.—Table of Harmonic Constants at New Indian Ports.

Mormugão.

Commence 0 h., March 16.

Year		1884–5.	1885–6.	1886–7.	Mean of 3 years.*
N	H =	0·427	0·438	0·427	0·431
	κ =	287	282	281	283
2N	H =	0·062	0·069	0·074	0·068
	κ =	239	263	239	247
λ	H =	*0·011	0·014	.....	0·013
	κ =	323	103	.....	213
ν	H =	0·153	0·104	0·018	0·092
	κ =	278	254	233	255
μ	H =	0·062	0·042	0·058	0·054
	κ =	247	246	248	247
R	H =	.....	0·006	.....	0·006 (1)
	κ =	.....	138	.....	138
T	H =	.....	0·068	.....	0·068 (1)
	κ =	.....	278	.....	278
MS	H =	0·022	0·028	0·025	0·025
	κ =	60	67	44	57
2SM	H =	0·002	0·008	0·007	0·004
	κ =	201	138	70	137
MN	H =	0·045	0·057	0·022	0·041
	κ =	343	342	337	341
MK	H =	0·019	0·035	0·039	0·031
	κ =	335	54	108	46
2MK	H =	0·009	0·006	0·005	0·007
	κ =	351	30	92	37
Mm	H =	0·048	0·029	0·015	0·031
	κ =	75	359	286	0
Mf	H =	0·048	0·075	0·089	0·071
	κ =	14	14	11	13
MSf	H =	0·021	0·057	0·041	0·040
	κ =	151	279	354	261
Sa	H =	0·306	0·165	0·291	0·254
	κ =	307	333	328	323
Ssa	H =	0·075	0·055	0·133	0·088
	κ =	163	68	147	126

* Except where noted thus (1), where this represents the number of years.

III.—Table of Harmonic Constants at New Indian Ports.

Cochin.

Commence at 0 h., January 25.

Year		1886–7.	1887–8.	Mean.	Year		1886–7.	1887–8.	Mean.
S_1	H =	0·031	0·039	0·035	N	H =	0·153	0·175	0·164
	κ =	161	227	194		κ =	301	300	300
S_2	H =	0·256	0·270	0·263	2N	H =	0·014	0·022	0·018
	κ =	26	37	31		κ =	274	185	230
S_4	H =	0·006	0·008	0·007	λ	H =	0·018	....	0·018
	κ =	203	138	171		κ =	321	....	321
S_6	H =	0·007	0·005	0·006	ν	H =	0·033	0·053	0·043
	κ =	226	222	224		κ =	355	334	345
S_8	H =	0·002	0·002	0·002	μ	H =	0·009	0·032	0·021
	κ =	162	297	230		κ =	168	204	186
M_1	H =	0·010	0·008	0·009	R	H =	....	....	....
	κ =	5	87	46		κ =	....	....	....
M_2	H =	0·731	0·731	0·731	T	H =	....	0·058	0·058
	κ =	332	330	331		κ =	....	9	9
M_3	H =	0·005	0·004	0·005	MS	H =	0·020	0·018	0·019
	κ =	159	265	212		κ =	135	143	139
M_4	H =	0·028	0·025	0·027	2SM	H =	0·004	0·009	0·007
	κ =	76	64	70		κ =	324	129	226
M_6	H =	0·009	0·011	0·010	MN	H =	0·023	0·014	0·019
	κ =	95	80	88		κ =	102	65	83
M_8	H =	0·002	0·003	0·003	MK	H =	0·037	0·025	0·031
	κ =	287	12	330		κ =	131	138	135
O	H =	0·306	0·326	0·316	2MK	H =	0·017	0·021	0·019
	κ =	58	56	57		κ =	107	108	108
K_1	H =	0·586	0·602	0·594	Mm	H =	0·014	0·035	0·025
	κ =	51	53	52		κ =	50	112	81
K_2	H =	0·089	0·063	0·076	Mf	H =	0·070	0·072	0·071
	κ =	26	21	23		κ =	355	36	16
P	H =	0·163	0·175	0·169	MSf	H =	0·037	0·042	0·040
	κ =	52	43	48		κ =	293	311	302
J	H =	0·026	0·039	0·033	Sa	H =	0·309	0·418	0·364
	κ =	77	49	63		κ =	313	296	305
Q	H =	0·068	0·082	0·075	Ssa	H =	0·134	0·161	0·148
	κ =	60	62	61		κ =	154	161	157
L	H =	0·027	0·041	0·034					
	κ =	24	332	358					

III.—Table of Harmonic Constants at New Indian Ports.

Galle.

Commence 0 h., April 1.

Year	1884–5.	1885–6.	1886–7.	Mean of 3 years.
S_1 H =	0·011	0·012	0·031	0·018
S_1 κ =	66	75	28	56
S_2 H =	0·357	0·357	0·370	0·361
S_2 κ =	97	94	92	94
S_4 H =	0·002	0·004	0·002	0·003
S_4 κ =	205	246	253	234
S_6 H =	0·001	0·000	0·004	0·002
S_6 κ =	264	135	106	168
S_8 H =	0·001	0·001	0·001	0·001
S_8 κ =	197	259	274	243
M_1 H =	0·010	0·008	0·004	0·007
M_1 κ =	225	245	333	268
M_2 H =	0·526	0·525	0·530	0·527
M_2 κ =	60	57	55	57
M_3 H =	0·014	0·012	0·014	0·013
M_3 κ =	166	161	150	159
M_4 H =	0·009	0·011	0·013	0·011
M_4 κ =	171	164	166	167
M_6 H =	0·004	0·003	0·003	0·003
M_6 κ =	2	336	24	1
M_8 H =	0·002	0·002	0·001	0·002
M_8 κ =	285	212	255	251
O H =	0·044	0·052	0·046	0·047
O κ =	79	79	78	79
K_1 H =	0·165	0·165	0·168	0·166
K_1 κ =	20	18	16	18
K_2 H =	0·093	0·089	0·154	0·112
K_2 κ =	92	104	101	99
P H =	0·053	0·049	0·037	0·046
P κ =	27	15	24	22
J H =	0·010	0·006	0·012	0·009
J κ =	69	53	355	39
Q H =	0·023	0·034	0·028	0·028
Q κ =	89	96	95	93
L H =	0·036	0·028	0·042	0·035
L κ =	67	7	80	51

III.—Table of Harmonic Constants at New Indian Ports.

Galle.

Commence 0 h., April 1.

Year		1884–5.	1885–6.	1886–7.	Mean of 3 years.*
N	H =	0·053	0·066	0·054	0·058
	κ =	47	42	45	45
2N	H =	0·007	0·020	0·009	0·012
	κ =	209	66	149	141
λ	H =	0·018	0·012	.....	0·015 (2)
	κ =	101	18	.....	59
ν	H =	0·048	0·038	0·013	0·033
	κ =	67	16	351	25
μ	H =	0·025	0·025	0·026	0·025
	κ =	102	106	100	103
R	H =	.....	0·018	.....	0·018 (1)
	κ =	.....	358	.....	358
T	H =	.....	0·041	.....	0·041 (1)
	κ =	.....	59	.....	59
MS	H =	0·006	0·006	0·009	0·007
	κ =	313	241	238	264
2SM	H =	0·007	0·012	0·008	0·009
	κ =	24	340	320	348
MN	H =	0·026	0·013	0·024	0·021
	κ =	165	229	189	194
MK	H =	0·005	0·008	0·005	0·006
	κ =	284	28	127	266
2MK	H =	0·002	0·001	0·003	0·002
	κ =	135	96	82	104
Mm	H =	0·067	0·017	0·017	0·034
	κ =	22	337	340	353
Mf	H =	0·020	0·027	0·066	0·038
	κ =	12	39	339	10
MSf	H =	0·013	0·013	0·030	0·019
	κ =	324	133	268	242
Sa	H =	0·377	0·287	0·346	0·337
	κ =	314	330	312	319
Ssa	H =	0·097	0·089	0·142	0·109
	κ =	125	102	122	116

* Except where noted thus (2), where this represents the number of years.

III.—Table of Harmonic Constants at New Indian Ports.

Colombo.

Commence 0 h., February 1.

Year		1884–5.	1885–6.	1886–7.	Mean of 3 years.
S_1	H =	0·018	0·030	0·003	0 017
	κ =	62	60	143	88
S_2	H =	0·362	0·389	0·404	0·385
	κ =	100	101	90	97
S_4	H =	0·004	0·004	0·004	0·004
	κ =	212	248	226	229
S_6	H =	0·002	0·002	0·002	0·002
	κ =	189	214	144	182
S_8	H =	0·001	0·001	0·000	0·001
	κ =	236	106	108	150
M_1	H =	0·008	0·013	0·006	0 009
	κ =	57	192	289	179
M_2	H =	0·546	0·563	0·590	0·566
	κ =	53	54	46	51
M_3	H =	0·015	0·015	0·014	0·015
	κ =	169	166	161	166
M_4	H =	0·015	0·014	0·017	0·015
	κ =	180	174	165	173
M_6	H =	0·002	0·003	0·005	0·003
	κ =	76	63	346	42
M_8	H =	0·000	0·001	0·000	0·000
	κ =	54	228	146	143
O	H =	0·093	0·101	0·091	0·095
	κ =	64	67	59	64
K_1	H =	0·237	0·231	0·239	0·236
	κ =	36	36	29	34
K_2	H =	0·072	0·104	0·126	0·101
	κ =	109	82	85	92
P	H =	0·082	0·062	0·068	0·071
	κ =	34	12	30	25
J	H =	0 030	0·006	0·013	0·016
	κ =	37	60	2	33
Q	H =	0·029	0·027	0·031	0·029
	κ =	81	88	82	84
L	H =	0·028	0·018	0·038	0·028
	κ =	54	46	64	55

III.—Table of Harmonic Constants at New Indian Ports.

Colombo.

Commence 0 h., February 1.

Year		1884–5.	1885–6.	1886–7.	Mean of 3 years.
N	H =	0·063	0·050	0·073	0·062
	κ =	29	47	30	35
2N	H =	0·011	0·012	0·008	0·010
	κ =	51	123	16	63
λ	H =	0·024	0·032	0·016	0·024
	κ =	59	56	16	44
ν	H =	0·023	0·014	0·011	0·016
	κ =	39	50	76	55
μ	H =	0·020	0·017	0·018	0·018
	κ =	106	97	122	108
R	H =	....	0·059	....	0·059 (1)
	κ =	....	340	....	340
T	H =	....	0·041	....	0·041 (1)
	κ =	....	353	....	353
MS	H =	0·005	0·008	0·009	0·007
	κ =	258	268	260	262
2SM	H =	0·008	0·005	0·008	0·007
	κ =	280	349	357	329
MN	H =	0·031	0·014	0·009	0·018
	κ =	252	256	262	257
MK	H =	0·004	0·002	0·007	0·004
	κ =	154	107	27	96
2MK	H =	0·005	0·002	0·005	0·004
	κ =	182	83	87	117
Mm	H =	0·013	0·035	0·040	0·039
	κ =	18	321	24	1
Mf	H =	0·033	0·064	0·049	0·049
	κ =	321	14	344	346
MSf	H =	0·014	0·012	0·026	0·017
	κ =	36	60	275	4
Sa	H =	0·328	0·267	0·323	0·306
	κ =	309	327	315	317
Ssa	H =	0·123	0·060	0·155	0·113
	κ =	128	83	122	111

* Except where noted thus (1), where this represents the number of years.

III.—Table of Harmonic Constants at New Indian Ports.

Cocanada.

Commence 0 h., March 31.

Year		1886–7.	1887–8.	Mean of 2 years.
S_1	H =	0·036	0·037	0·037
	κ =	93	77	85
S_2	H =	0·644	0·628	0·636
	κ =	285	286	285
S_4	H =	0·003	0·007	0·005
	κ =	126	147	136
S_6	H =	0·003	0·004	0·004
	κ =	205	160	182
S_8	H =	0·003	0·003	0·003
	κ =	221	83	152
M_1	H =	0·019	0·023	0·021
	κ =	341	342	341
M_2	H =	1·486	1·545	1·516
	κ =	252	252	252
M_3	H =	0·006	0·009	0·008
	κ =	346	20	3
M_4	H =	0·026	0·027	0 027
	κ =	109	106	107
M_6	H =	0·014	0·016	0·015
	κ =	98	101	99
M_8	H =	0·002	0·002	0·002
	κ =	66	295	1
O	H =	0·133	0·137	0·135
	κ =	333	332	333
K_1	H =	0·347	0·352	0 350
	κ =	340	338	339
K_2	H =	0·175	0·169	0·172
	κ =	286	284	285
P	H =	0·099	0·089	0·094
	κ =	344	343	344
J	H =	0·028	0·036	0·032
	κ =	338	336	337
Q	H =	0·017	0·008	0·018
	κ =	36	21	28
L	H =	0·075	0·082	0·079
	κ =	272	235	254

Year		1886–7.	1887–8.	Mean of 2 years.*
N	H =	0·308	0 326	0·317
	κ =	244	242	243
2N	H =	0·043	0·060	0·052
	κ =	242	230	236
λ	H =	0·008	.....	0·008 (1)
	κ =	83	.....	83
ν	H =	0·071	0·018	0·045
	κ =	191	303	247
μ	H =	0·019	0·032	0·026
	κ =	257	264	260
R	H =			
	κ =			
T	H =	.....	0·004	0·064 (1)
	κ =	.....	294	294
MS	H =	0·014	0·023	0·019
	κ =	131	145	138
2SM	H =	0·015	0·018	0·017
	κ =	215	181	198
MN	H =	0·031	0·041	0·036
	κ =	120	135	128
MK	H =	0·024	0·024	0 024
	κ =	296	16	336
2MK	H =	0·011	0·010	0·011
	κ =	326	318	322
Mm	H =	0·029	0·076	0·053
	κ =	198	290	244
Mf	H =	0 078	0·095	0·087
	κ =	55	196	126
MSf	H =	0·033	0·023	0·028
	κ =	19	125	72
Sa	H =	0·853	0·671	0·762
	κ =	200	199	199
Ssa	H =	0·403	0·522	0·463
	κ =	109	99	104

* Except where noted thus (1), where this represents the number of years.

III.—Table of Harmonic Constants at New Indian Ports.

	Chittagong. Commence 0 h., June 6.			*Akyab.* Com. 0 h., May 9.
Year	1886-7.	1887-8.	Mean of 2 years.	1887-8.
S_1 { H =	0·060	0·056	0·058	0·042
S_1 { κ =	120	127	123	84
S_2 { H =	1·568	1·553	1·561	1·118
S_2 { κ =	68	68	68	310
S_4 { H =	0·049	0·053	0·051	0·006
S_4 { κ =	55	63	59	209
S_6 { H =	0·010	0·010	0·010	0·008
S_6 { κ =	131	125	128	107
S_8 { H =	0·002	0·002	0·002	0·008
S_8 { κ =	217	147	182	113
M_1 { H =	0·025	0·022	0·024	0·016
M_1 { κ =	23	47	35	342
M_2 { H =	4·428	4·440	4·434	2·540
M_2 { κ =	35	35	35	280
M_3 { H =	0·039	0·044	0·042	0·020
M_3 { κ =	218	198	208	11
M_4 { H =	0·421	0·395	0·408	0·006
M_4 { κ =	342	344	343	290
M_6 { H =	0·143	0·149	0·146	0·023
M_6 { κ =	195	188	192	132
M_8 { H =	0·035	0·034	0·035	0·006
M_8 { κ =	127	112	119	143
O { H =	0·295	0·289	0·292	0·183
O { κ =	12	16	14	338
K_1 { H =	0·582	0·576	0·579	0·443
K_1 { κ =	22	20	21	344
K_2 { H =	0·438	0·397	0·418	0·317
K_2 { κ =	71	66	68	304
P { H =	0·192	0·195	0·194	0·141
P { κ =	26	31	29	347
J { H =	0·053	0·027	0·040	0·021
J { κ =	51	99	75	1
Q { H =	0·016	0·025	0·021	0·002
Q { κ =	328	359	343	169
L { H =	0·425	0·399	0·412	0·108
L { κ =	60	39	50	291

III.—Table of Harmonic Constants at New Indian Ports.

	Chittagong. Commence 0 h., June 6.			*Akyab.* Com. 0 h., May 9.
Year	1886–7.	1887–8.	Mean of 2 years.*	1887–8.
N { H =	0·869	0·841	0·855	0·520
N { κ =	24	25	24	271
2N { H =	0·081	0·080	0·055	0·052
2N { κ =	19	294	337	450
λ { H =	0·207	.....	0·207 (1)	
λ { κ =	61	.....	61	
μ { H =	0·402	0·295	0·349	0·053
μ { κ =	24	2	13	202
ν { H =	0·268	0·276	0·272	0·017
ν { κ =	200	206	203	225
R { H =				
R { κ =				
T { H =	.....	0·139	0·139 (1)	
T { κ =	.....	246	246	
MS { H =	0·355	0·344	0·350	0·012
MS { κ =	18	24	21	313
2SM { H =	0·129	0·138	0·133	0·041
2SM { κ =	299	303	301	198
MN { H =	0·143	0·088	0·116	0·102
MN { κ =	246	275	261	106
MK { H =	0·131	0·102	0·117	0·016
MK { κ =	310	338	324	220
2MK { H =	0·049	0·043	0·046	0·012
2MK { κ =	263	263	263	28
Mm { H =	0·075	0·177	0·126	0·026
Mm { κ =	339	9	354	284
Mf { H =	0·181	0·173	0·177	0·081
Mf { κ =	40	343	12	289
MSf { H =	0·432	0·459	0·446	0·046
MSf { κ =	39	42	41	58
Sa { H =	1·666	1·435	1·551	0·950
Sa { κ =	137	132	134	146
Ssa { H =	0·178	0·105	0·142	0·252
Ssa { κ =	217	73	325	129

* Except where noted thus (1), where this represents the number of years.

III.—Table of Harmonic Constants at New Indian Ports.

Elephant Point (New Site).

Commence 0 h., January 1 of each year except for 1887–8 (June 12, 1887).

Year	1884.	1885.	1886.	1887.	1887–8.	Mean of 5 years.
S_1 { H =	0·140	0·082	0·082	0·075	0·101	0·096
S_1 { κ =	91	126	128	114	112	114
S_2 { H =	2·384	2·307	2·365	2·366	2·395	2·381
S_2 { κ =	140	140	140	140	140	140
S_4 { H =	0·092	0·088	0·078	0·081	0·081	0·084
S_4 { κ =	181	177	174	176	173	176
S_6 { H =	0·013	0·007	0·010	0·011	0·008	0·010
S_6 { κ =	294	262	296	272	258	277
S_8 { H =	0·009	0·005	0·002	0·003	0·001	0·004
S_8 { κ =	307	284	340	38	63	351
M_1 { H =	0·039	0·009	0·015	0·039	0·038	0·028
M_1 { κ =	26	125	55	64	73	69
M_2 { H =	5·876	5·890	5·897	5·907	5·941	5·902
M_2 { κ =	102	104	103	103	104	103
M_3 { H =	0·021	0·026	0·027	0·040	0·031	0·029
M_3 { κ =	15	337	323	305	286	325
M_4 { H =	0·270	0·289	0·275	0·290	0·280	0·281
M_4 { κ =	79	88	91	90	91	88
M_6 { H =	0·252	0·241	0·239	0·242	0·246	0·244
M_6 { κ =	339	338	338	332	334	336
M_8 { H =	0·107	0·101	0·104	0·104	0·104	0·104
M_8 { κ =	324	334	335	326	323	328
O { H =	0·344	0·323	0·323	0·313	0·312	0·323
O { κ =	6	8	7	5	6	6
K_1 { H =	0·723	0·737	0·751	0·761	0·760	0·746
K_1 { κ =	20	19	19	18	18	19
K_2 { H =	0·980	0·716	0·589	0·710	0·768	0·752
K_2 { κ =	120	135	136	144	147	137
P { H =	0·162	0·189	0·195	0·223	0·195	0·193
P { κ =	18	32	36	31	33	30
J { H =	0·029	0·064	0·011	0·025	0·028	0·030
J { κ =	77	103	107	61	89	87
Q { H =	0·043	0·024	0·004	0·030	0·029	0·026
Q { κ =	23	329	279	4	39	351
L { H =	0·440	0·250	0·412	0·448	0·423	0·395
L { κ =	117	132	139	126	120	127

III.—Table of Harmonic Constants at New Indian Ports.

Elephant Point (New Site).

Commence 0 h., January 1 of each year except for 1887–8 (June 12, 1887).

Year		1884.	1885.	1886.	1887.	1887–8.	Mean of 5 years.*
N	H =	0·961	1·052	1·145	1·207	1·188	1·111
	κ =	90	86	86	88	91	88
2N	H =	0·281	0·205	0·102	0·105	0·197	0·178
	κ =	87	85	144	327	14	59
λ	H =	0·188	0·178	.....	.....	.....	0·183 (2)
	κ =	162	144	.....	.....	.....	153
ν	H =	0·182	0·137	0·346	0·416	0·313	0·269
	κ =	68	122	123	95	67	95
μ	H =	0·346	0·391	0·342	0·329	0·382	0·358
	κ =	273	293	288	302	302	292
R	H =	.....	0·077	.....	.....	.....	0·077 (1)
	κ =	.....	104	.....	.....	.....	104
T	H =	.....	0·318	.....	0·142	.....	0·230 (2)
	κ =	.....	93	.....	185	.....	139
MS	H =	0·310	0·296	0·292	0·277	0·281	0·291
	κ =	122	128	126	129	131	127
2SM	H =	0·168	0·112	0·131	0·134	0·138	0·136
	κ =	42	35	35	39	40	38
MN	H =	0·285	0·198	0·126	0·199	0·196	0·191
	κ =	34	45	36	80	136	66
MK	H =	0·078	0·055	0·134	0·151	0·047	0·092
	κ =	66	344	3	36	47	27
2MK	H =	0·069	0·076	0·069	0·073	0·032	0·064
	κ =	351	353	354	357	350	353
Mm	H =	0·120	0·120	0·075	0·056	0·107	0·096
	κ =	349	7	0	347	351	355
Mf	H =	0·190	0·120	0·148	0·044	0·037	0·108
	κ =	10	24	13	108	20	35
MSf	H =	0·226	0·245	0·199	0·221	0·170	0·212
	κ =	56	53	27	37	30	41
Sa	H =	0·812	0·878	0·918	0·764	0·845	0·842
	κ =	117	141	152	141	149	140
Ssa	H =	0·134	0·107	0·141	0·150	0·115	0·129
	κ =	204	219	122	89	114	150

* Except where noted thus (2), where this represents the number of years.

"The Structural Arrangement of the Mineral Matters in Sedimentary and Crystalline Pearls." By GEORGE HARLEY, M.D., F.R.S. Received March 6,—Read March 28, 1889.

(Abstract.)

The author began by giving a sketch of what has hitherto been written on pearl structure, and pointed out that since Rondelet* threw out the idea that pearls are merely diseased concretions occurring in the Mollusca in the same way as other morbid calculi occur in the Mammalia, and Réaumur† said they are misplaced pieces of organised shell, in the same way as loose cartilages in human joints are misplaced portions of the cartilaginous structures surrounding them, the opinions of all subsequent writers have but oscillated between these two antagonistic theories. The two chief exponents of pearl structure in modern times—Meckel in 1856,‡ and Möbius§ in 1858—have ranged themselves on opposite sides; the former advocating the views of Rondelet, the latter espousing those of his opponent Réaumur; while Bronn,|| a still more recent writer, has evidently a difficulty in deciding which of the theories is the correct one. For, while the whole tenour of his remarks leads one to believe that he favours the shell-formation theory of Réaumur, he speaks of pearls as being lime concretions (*Kalk-Konkretionen*), and pearl-like calculi (*Perlen-artige Konkrementen*), which he would be unlikely to do were he firmly convinced in the validity of Réaumur's theory.

It is thus seen that it is still an unsettled question whether pearls belong to the animal or the mineral kingdom. And no one who has worked at the subject, and knows its intricacy, can be the least surprised at our very best authorities being still unable to decide as to which of the two antagonistic theories is the best. For, while there are undoubtedly many potent data in favour of each view, there are, at the same time, not a few grave objections in the way of a ready acceptance of either, as the following facts prove:—

1. As was demonstrated by the exhibition of specimens, many pearls (off-coloured ones) of the sedimentary variety so closely resemble the carbonate of lime calculi met with in the Mammalia, and the carbonate of lime pisolite concretions from the mineral waters of Carlsbad, as to be absolutely undistinguishable from them, either by the naked eye or with the aid of a microscope.

* 'Univers. Aquatil. Hist.,' 1554.

† 'Mém. de l'Acad. d. Sci.,' 1717.

‡ 'Mikrogeologie: Ueber die Concrementen im thierischen Organismus,' Berlin, 1856.

§ 'Die Echten Perlen: ein Beitrag zur Luxus, Handels- und Naturgeschichte derselben,' Hamburg, 1858.

|| 'Weichthiere,' Leipzig, 1862, "Perlen-Bildung," p. 422.

2. All these three kinds of concretions are formed in consecutive strata round a nucleus, and the strata are not only independent and separable from each other, but so loosely adherent that they can be shelled off like the different layers of an onion, leaving the subjacent layers so perfect that their enucleated portions constitute for themselves a perfect pearl, a perfect calculus, or a perfect pisolite concretion.

3. By the specimens exhibited, it was shown that the outward appearance of a pearl is in no case a reliable criterion of its internal structure, a dull-white sedimentary pearl appearing exactly the same under the microscope as a beautifully brilliant iridescent one; while a black-coloured pearl may possess in its interior a snow-white pearl, another of the purest water may consist of nothing but a dirty greasy lump of river clay, being in reality merely thinly coated over with iridescent pearl substances.

4. Thin sections of sedimentary pearls, when viewed with a high power in a good light, have all more or less a granular appearance, in general best marked along the lines of stratification.

5. All sedimentary pearls have nuclei round which the concentric strata are regularly arranged, the nucleus being sometimes small, sometimes exceedingly large in proportion to the size of the pearl. And, contrary to the opinion of De Filippi,* who asserts that the nucleus is invariably an *Entozoon*, it may consist of inorganic as well as of organic foreign material—a pellet of clay, a particle of sand, a fragment of bone or of iron, a piece of seawood, or even an entire animal.

In the second division of his subject, the author demonstrated by specimens that just as a sedimentary pearl closely resembles the sedimentary concretions met with in the mineral as well as in the animal world, so in like manner the crystalline form of pearl has its exact counterpart not alone in organic cholesterin gall-stones and carbonate of lime calculi, but in mineral nodules of wavellite rock and balls of iron pyrites.

As regards the nuclei of crystalline pearls, it was shown that in most instances they are identical with those met with in the sedimentary variety of pearls, though sometimes no nucleus whatever is to be met with even after prolonged search. Consequently, the author inclines to believe that crystalline pearls in some instances begin (as in the case of natural as well as artificially prepared calculi of the carbonate of lime, as Rainey† showed), by the mere aggregation and coalescence of mineral molecules.

The author also called attention to Bronn's statement that pearls have often a crystalline nucleus, adducing evidence in favour of the

* Müller's 'Archiv,' 1856, p. 490.

† 'The Formation of Bone and Shell Structures,' 1858, pp. 11, 12, and 35.

view that the so-called crystalline nucleus is in many cases but the centre, and therefore really an integral part of the pearl's crystalline structure; while, again, in other instances, the appearance is due to the section having been carried a little on one side of the pearl's centre, and a consequent cutting across of some of the hexagonal basaltic-like prisms, producing in the centre of the pearl a tesselated crystalline appearance.

Bronn further asserts that in the centre of pearls are occasionally found cells containing calcspar, which has arrived there by a process of infiltration from without. This appearance, the author thought, could be better explained on other grounds, and he exhibited the microscopic section of an oxalate of lime human urinary calculus with a crystalline centre, surrounded by a sedimentary zone exactly as is seen in some pearls, but where no idea of infiltration could possibly be entertained.

A good section of a crystalline pearl, while showing basaltic-like prisms radiating from the centre to the circumference, equally well shows that it, too, like the sedimentary variety, is formed in concentric layers of stratification. In this respect it not only bears a marked similarity to the carbonate of lime concretions formed artificially in gum-water, but an equal analogy to the sulphate of baryta stalactites found in the caves of Derbyshire, where, of course, it is impossible to attribute the structural arrangement of their mineral matters to the influence of vital energy.

Microscopic sections of crystalline pearls not alone convey the idea that the prisms branch and interlace with each other, but that they are in some instances of a fusiform shape, like the so-called fusiform cells Carpenter described in shell structures; but these appearances, as the author demonstrated, are simply due to the section having cut the prisms across at different angles. Moreover, the prisms are striated, but this striation, as well as the feathery frond-like appearance some of them present, does not, he thinks, exist in the mineral matter, but in the animal membrane which surrounds each individual prism, and which, as he previously showed, is an integral part of the pearl, amounting to 5·94 per cent. of its total weight.* The feather-like appearance, he fancies, is merely due to a wrinkling of the dried animal matter. It was further pointed out that each individual prism is made up of a number of brick-like segments. Some with a high power appearing striated (*b*), others granular (*a*) with knob-like bodies at their corners, and tube-like lines near their layers of stratification.

The discussion of the arrangement of the animal matter is deferred until after the mineral structure of hybrid, so-called cocoa-nut, and fossil pearls has been considered.

* "The Chemical Composition of Pearls," 'Roy. Soc. Proc.,' vol. 48, p. 461.

OBITUARY NOTICES OF FELLOWS DECEASED.

Dr. Parkinson was born in 1823 near Keighley, in Yorkshire, and died in 1889 at his residence in Cambridge.

His father died when he was a boy, and left to the widow the difficult task of bringing up a large family on a very narrow income. Coming up to Cambridge in October, 1841, he began his college life with an examination for a sizarship. One of his competitors, who sat just in front of him at this examination, still remembers with wonder how he finished his papers long before the others, and how he sat at his ease with his back against the wall for a long time. The success with which he thus began his college life was due to his own energy and talent, for as a boy he had but limited opportunities for study, and the same energy carried him on successfully throughout his life.

As a lad without independent means, it was necessary for him to succeed, and accordingly he prepared to do his best at the final University examination. He had most formidable competitors, and the contest therefore excited considerable interest at the time. It was a very different thing from his first skirmish for the sizarship, and it was only after a hard fight that the Johnian was declared to be the Senior Wrangler. The scene is described in a lively manner by a contemporary, an American who resided for five years in an English University. Such descriptions are outside the object of the present memoir, but it helps us to understand the skill of the competitors to learn that the Senior Wrangler did more than two-thirds of the problems set in all the three problem papers. Such at least was the current report of the day.

The Smith's Prize examination was at that time so arranged that the element of speed did not enter into it to the same extent as into the Tripos. The subjects of examination were in general beyond the reach of ordinary undergraduates. Here the places were reversed, and Parkinson stood second in the list. The contest, however, was well sustained, for in one paper the two first competitors obtained respectively 63 and 55 per cent. of the marks.

As soon as he had taken his degree he devoted himself to an academical life. He had begun to take pupils even before his degree, and now continued to do so with great success. Three of his pupils, viz., Besant, Sprague, and Finch, were the Senior Wranglers in the years 1850, 1853, and 1857; another pupil, the Right Hon. L. H. Courtney, M.P., now Chairman of Committees and Deputy Speaker, was second in 1855. In 1864 he was appointed to be College Tutor. It was here he

found his real vocation in life, and worked at it until 1882. He looked after his pupils in a business-like way, with mingled firmness and kindness, and they reciprocated by giving him their confidence. Some of them have afterwards described how kindly he had assisted them with means, and by his influence started them successfully on their journey through life. His remembrance of his pupils did not come to an end when they had passed from his care, but he and they remained ever mutual friends. In this way he became well known outside the University, his name and influence attracting many students to his College. When he married in 1871, he expected, as the custom then was, that he would lose both his fellowship and tutorship. But the College would not part with so valuable a tutor. The Master and Seniors requested him to continue in his office of tutor though residing in his own house. This was a compliment of which he was justly proud. He continued to act as tutor for eleven more years, and was then elected a second time to a fellowship in his College. He, however, did not retain the dividends of this office, but of his own free will gave them up to the College. Later on he gave £500 for the Church at Walworth, as this is the College mission belonging to St. John's, not the only gift of his to this district.

Dr. Parkinson took his B.D. degree in 1855, and became Doctor of Divinity in 1869. He acted as curate shortly after his degree in a neighbouring village, but the pressure of his other duties prevented him from taking much more active work. He was on the Commission of the Peace for the borough for several years. He was elected a Member of the Cambridge Philosophical Society in 1845, a Member of the Royal Astronomical Society in 1853, and a Fellow of the Royal Society in 1870. He married Miss Whateley, of Edgbaston Hall, in 1871.

Dr. Parkinson was not a writer of many books. His treatises on Elementary Mechanics and on Optics were published while engaged in tuition. They do not contain any novelties, but were written because experience had shown him that students had found difficulties in these subjects, which he thought he could remove. Their commercial success is therefore a good test of their excellence, and of this there can be no doubt. They came into general use in the University, and for several years they were very generally read. They each passed through several editions. They have, however, now been superseded by newer books with methods more adapted to the wants of the present day.

Dr. Parkinson took a prominent part in University affairs. He was Examiner for the Mathematical Tripos in 1849, and Moderator in 1852. He served as Senior Proctor in 1864. He was a Member of many syndicates appointed to consider weighty questions as they arose. For example, he served on the important syndicate which

in 1867 enlarged the scope of the Mathematical Tripos. For three successive periods of four years each, beginning in 1866, he was elected a Member of the Council of the Senate, his popularity in the University being shown by the large majorities by which he headed the poll at each of his two re-elections. He was one of the first appointed Members of the General Board of Studies constituted by the Statutes of 1882. He was also elected by the "Colleges in common" in 1882, and on the expiration of his period of service again in 1886 as one of their first representatives on the Financial Board of the University. He was a Member, and for the most part Chairman, of the Board of Examination from its establishment in 1873 till within a few months of his death, when failing health compelled him to resign this and other offices, the duties of which he felt himself no longer able to discharge.

In his public capacity his wise and prudent counsels, his able administration and management, his thoroughness and directness of purpose, were universally recognised. In his private capacity a wide circle of friends will long remember his genial heartiness, his constant and kindly thoughtfulness.

E. J. R.

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END OF FORTY-FIFTH VOLUME.

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